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THE AUDITORY MEMORY AFTER-IMAGE

By CLAIRE COMSTOCK DIMMICK

The purpose of this investigation has been to secure a description of the memory after-image in order to determine whether it deserves a place of its own in a classification of images or whether, as has been suggested,[1] it is a blending of other images and merely appears to be unitary. The auditory rather than the visual field has been chosen to avoid the difficulties of dealing with positive and negative after-images; for whether or not there is any after-effect of auditory stimulation, it seems to have been conclusively shown that "there is no positive after-image of tone, analogous to the positive after-image of vision."[2]

Following the suggestion made by Titchener that "the name of memory after-image has been given to an experience which is most familiar, perhaps, in the taking of dictation",[3] we began our experimental work with the following instructions: "I shall dictate to you some sentences which you are to take down as accurately as possible. When I say 'Now', stop writing and describe as well as you can your experiences in taking the dictation." In the preliminary experiments *E* read from any book at hand; but for the systematic observations sentences, all of a standard length, taken from Locke's "Essay Concerning Human Understanding" were used. No effort was made to select complete sentences, for we desired to have the attention of the *O*s on the sound rather than on the meaning, so that the sentences were somewhat obscure in meaning and incomplete as to

[1] L. J. Martin, Die Projektionsmethode u. d. Lokalisation visueller u. a. Vorstellungsbilder, *Zeits f. Psych.*, 61, 1912, 346ff.

[2] H. G. Bishop, The Positive After-Image in Audition, this JOURNAL, 32, 1921, 325.

[3] E. B. Titchener, *A Beginner's Psychology*, 1918, 74.

form. *E* read fast enough to keep from 4 to 6 words ahead of *O*, who was required to take down every word, though he was permitted to use abbreviations. When *E* said "Stop," she was always from 4 to 6 words ahead of *O*; that is, there were 4 to 6 words of the sentence that *O* had not yet written.

There were 5 *O*s: Mr. DeGowan (De), Miss Stewart (St), Miss Sutherland (Su), all undergraduate students in psychology at the University of Michigan; Mr. J. D. Finlayson (F), and Dr. F. L. Dimmick (Di), instructors in psychology. All the *O*s, with the exception of St who gave only 8 hours to the experiment, worked 2 hours a week during the second semester.

Results

All of the *O*s reported the presence and use of auditory imagery in the taking of dictation.

"There seem to be 2 parallel processes . . . . the last words of a group of words seem to stick in my mind and reëcho at the same time that you are talking" (De).

"I could hear the last three words just as you said 'Stop' repeated in auditory imagery" (Di).

"When I stopped the rest of the sentence sounded vividly in mind. What I heard was your voice repeating the rest of the sentence" (F).

"First I heard you, then I wrote from hearing you again" (Su).

"I hear the words after you've stopped; I hear it all to the end" (St).

After a number of observations had been made, it was obvious from a consideration of the reports that the most frequently and easily observed auditory images were those which came immediately after *E* said "Stop." All of the *O*s reported that the sentences finished themselves out in auditory imagery, though they had been instructed to stop writing. This imagery was not called up, but was "just there." Some of the inexperienced *O*s, indeed, did not report in terms of imagery, but merely said that they "heard something." All of the *O*s said that they were surprised to hear the "Stop" at the end of the words (in imagery) for it was obviously irrelevant to the meaning. The "tagging on" of this word indicates how closely the imagery copied the perception and how spontaneously it came.

Since the auditory image which came after the command to stop was invariably present and always in *E*'s voice, the *O*s were next asked to describe this image attributively. The procedure was the same as outlined above, except that *O* was asked to observe only the auditory image at the end of the dictation. Reports on the intensity, duration, pitch and richness[4] of the

[4]The category "Richness" was used to include those descriptive phrases which, while not strictly attributive, indicate the make-up or timbre of the tone, which was not further analysable.

auditory image were called for. With the exception of St, all *O*s gave at least 50 reports on each one of the above named attributes. St gave 25. A table of the results follows.

	INTENSITY					DURATION		PITCH		RICHNESS			
	Same	Slightly less	Less	Much less	Very much less	Same	*Longer	Same	Higher	Same	Slightly Less	Less	Much less
Su	29	32	2	—	—	46	4	49	1	33	13	4	—
De	38	16	—	—	—	14	36	51	—	34	10	6	—
Di	—	21	26	2	2	53	—	51	—	3	27	16	4
F	—	7	9	35	14	60	1	52	—	45	—	5	—
St	23	2	—	—	—	20	5	27	—	27	—	—	—

All descriptions of the image are in terms of its likeness to the perception, so that "Same" indicates of the same intensity as the perception; "Less" indicates of less intensity than the perception.

*The total duration of the image was lengthened by the 'dragging out' of the last word.

These reports show that the auditory image differs from the auditory perception chiefly with respect to the attributes of intensity and richness. Yet this difference is not great for all *O*s; Su, De, and St find the image as loud as (or only slightly less loud than) the perception, and all *O*s report that the image differs little from the perception in richness. F, whose images were much less in intensity than the perception, also reported later that his memory-images were very weak; whereas Su and St described their memory-images as being of moderate intensity. The duration of the image is the same as that of the perception, except in those cases where the dragging out of the last word made the total duration longer. The auditory memory after-image, then, under these conditions is an excellent copy of the perception in pitch and duration; and for three of the *O*s in intensity and richness, though two *O*s found the image less than the perception in these respects. Su, St, and De frequently reported that they could not note any attributive differences between the image and the perception and, indeed, would not have been able in many cases to distinguish between them except on a basis of stimulus difference.

The *O*s were also asked to report on the temporal course of the image. The only attributive changes described by any *O* were in 3 cases, in which the last word of the image lasted longer than the preceding words and decreased in intensity or faded out. All images came immediately or after a very brief interval after *E* said "Stop." For 3 *O*s the image came all at once or was "just there"; for 2 *O*s the image came "word by word" and went similarly, so that when each subsequent word came the preceding word was gone. The experimenter is inclined to believe that the "word by word" reports are the result of a logicizing rather than a purely descriptive attitude.

The question now arose how this image which we had been calling the memory after-image differed from the memory-image; if, perhaps, it might not be a memory-image. In order

to secure memory-images under conditions as similar as possible to those under which we studied the memory after-image, we instructed the *O*s as follows: "I shall read you a sentence. Write it down as accurately as you can. You will be given further repetitions until you can write it perfectly." The same sentences that had been used before were also used here. We hoped, by dictating the sentences to be learned, to put a premium on the use of auditory imagery in their revival; and by having them learned by means of written rather than spoken repetitions to decrease the use of verbal-motor imagery and imagery of the *O*s' own voices in the reproductions. We hoped to get a reproduction in terms of *E*'s voice so as to make the two kinds of images more comparable. After *O* had memorized 3 sentences in this way, the following instructions were given: "I shall read you a part of one of the sentences that you have memorized. Complete the sentence as accurately as you can. When I say 'Stop,' stop writing and describe your experiences in completing the sentence." Each sentence was divided into six parts, so that there were six places where *E* might stop and *O* supply the following phrase. As soon as *O* had written the first 2 words of the supplied phrase, *E* said "Stop." Samples of the types of reports secured under these conditions are given for each *O*.

"The completion didn't come immediately and I repeated the first part of the sentence. Then the completion came in the form of auditory imagery, partly in your voice, partly in mine" (Su).

"I had an auditory image, but I said it along with the image. I don't know whose voice the image was in" (Su).

"I had to repeat some of the preceding words. The completion came in the form of a verbal image with an auditory component. The verbal-motor part was the more prominent" (De).

"The completion came mostly in verbal-motor imagery until I got to the second word and it came in auditory imagery" (De).

"I got the phrase mainly in verbal-motor imagery with a very weak auditory accompaniment. I can't say whether the image was in *E*'s voice or not" (Di).

"I got a very weak auditory image but not in *E*'s voice. This is not at all comparable to the other experience (memory after-image)" (Di).

"The phrase came in just meaning; no imagery" (F).

"Suddenly the meaning came. Then the phrase came in verbal-motor imagery" (F).

"The completion came in verbal-motor imagery and auditory imagery of your voice. The verbal-motor imagery was the more important" (St).

The foregoing reports show that when the *O*s were left to themselves to recall a phrase presented auditorily the general tendency was to recall in terms of verbal-motor imagery. Auditory imagery, if present, was weak and subordinate to verbal-motor. Thus we did not secure the description of auditory imagery that we were hoping for, but we did get evidence that

these *O*s were not auditory-minded, so that the auditory images reported in the first part of the experiment did not occur because of any tendency to belong to the auditory type.

The instructions were now changed to provide for a report on auditory imagery and read as follows: "I shall read you part of one of the sentences you have learned. When I stop complete the sentence in auditory images of my voice. Report, however, upon the total experience from the 'Stop' to the disappearance of the auditory image." The reports given show that in most instances the phrase to be supplied did not come in auditory imagery but had "to be put into" auditory imagery.

"I said it until I could hear you saying it; it didn't come in auditory imagery. There was a feeling of effort" (Su).

"The phrase came immediately in your voice. I can stop this imagery anywhere and have it go on in your voice, but when I'm taking dictation I can't. I usually can't help myself but listen to it finish; but if I do stop it, it doesn't come back in your voice" (Su).

"I ran back over the sentence in verbal-motor imagery; I could by effort get a slight auditory image, but accompanying it was verbal-motor imagery. The auditory image was very faint but in your voice" (De).

"The last few words that you speak hang over in auditory imagery and verbal-motor imagery; then a verbal-motor image of the completion comes. Then I can get an auditory image by trying to accompany the verbal-motor imagery. I am always conscious of trying to imitate your voice" (De).

"I have to force the verbal-motor imagery into an imitation of your inflections. The other images (memory after-images) seem to persist from the perception. I get them without verbal-motor imagery" (De).

"I can put it into auditory imagery, but verbal-motor imagery is still there. If I were let alone I'd remember it that way and not auditorily. The verbal-motor images make the sounds into words" (Di).

"I thought the sentence through in verbal-motor imagery; then I put it into auditory imagery. It is always voluntary" (F).

"The completion didn't come in your voice, but I had to put it in it" (St).

Since all the *O*s could, though with effort, get an auditory reproduction, they were next asked to attend to the auditory image and to describe it attributively. It should perhaps be stated that the effort required was no greater than that usually necessary to call up an auditory image. The point we have been making above is that the *O*s were not of the auditory type though all could get auditory images. Inasmuch as there have been numerous studies of memory-images of all modalities and our time was limited, we did not try to make a throughgoing study of the auditory memory-image, but attempted merely to get from these *O*s a description of their auditory memory-images in order to have a basis for comparison with the images described in the first part of the experiment.

Accordingly we asked for only 10 to 20 reports from each *O*. These reports we have summarized.

For Su the intensity of the auditory memory image varied from "almost as intense as" to "moderately intense," the preponderance of cases being in the latter category; the duration, the pitch, and the richness were the same as in the spoken word. For De the intensity was either weak or very weak, the duration was the same as that of the verbal-motor imagery always accompanying it which could be prolonged at will, the pitch was the same as that of the perception, the richness was either less or very much less. For Di the intensity was for the most part weak, at times very weak, the duration and pitch were the same as those of the perception, the richness was always much less. For F the intensity varied between weak and very weak, the duration and pitch were the same as those of the perception, and the richness was sometimes the same as that of the perception and sometimes weak. For St the intensity varied between "almost as intense as", moderately intense, and weak; the duration and pitch were the same as in the perception; the richness was the same or "almost the same" as that of the perception.

In addition we secured reports on the localization of both the memory-image and the memory after-image. For Su both images were with one exception localized inside the head. Once the memory after-image was localized outside, above the head. For De the memory-image was "internal, in the front of the head," except once when it seemed peculiarly like *E*'s voice and was projected; the memory after-image was objective, external, projected out from a point about level with *E*'s head, and seemed to come from the direction in which *E* was sitting. For Di the memory-image was inside the head; the memory after-image came "from toward *E* though less definitely than the perception" and seemed partly external and partly internal, for there was "some ring in the ears themselves." For F the memory-image was inside the head; the memory after-image was outside the head and from the general direction of *E*. For St the memory-image was in general inside the head; the memory after-image was outside, though it once was reported as being "half and half."

If we compare the attributive description of the memory-image with that of the memory after-image we find that in no case was the intensity of the memory-image as great as that of the memory after-image for the same *O*. Su, whose auditory memory after-images were as loud or almost as loud as the perception, described her memory-images as only moderately intense, though a few of them were "almost as intense." De, who had reported over two-thirds of his memory after-images to be as loud as the perception, had only weak or very weak memory-images. The memory-images described by Di were weak in intensity whereas his memory after-images had been moderately loud. F's reports do not show so marked a difference, though he never had a memory-image better than "weak" while some of his memory after-images were only slightly less loud than the perception. For St the memory-images were less intense than the memory after-images. So far as intensity goes, then, we find a difference in the two kinds of images. The attributes of pitch and duration present no basis for distinction. Richness, in general, seems less in the memory-image than in the memory after-image. The memory after-image is localized by all but one *O* (Su) as outside the head, whereas all *O*s localize the memory-image inside the head. For all *O*s the memory

after-image is "just there," "persistent," while the memory-image comes only when tried for and is accompanied in most cases by a feeling of effort. Of the three bases for differentiation (1) attributes, (2) perceptive modes, (3) conditions of arousal, the first offers the least generally applicable criterion, though one which is valid for individual *O*s; the second a more reliable criterion under the conditions of this experiment; and the last the most positive.

Relation to Previous Investigations

The term memory after-image (Erinnerungsnachbild) was introduced by Fechner.[5] He describes experiences in the visual field only and bases his description of the memory after-image mainly on his own observations, though he records a few reports from others. For him a memory after-image came after a brief attentive glance at an object, when upon looking away or closing his eyes he saw for an instant very clearly an image of the object looked at in full color and outline. This, he says, he was unable to do in the case of objects he had seen a longer time before. The image, however, very quickly lost the definiteness of its outline and its color.

The conditions of our experiment have provided for attention of short duration to a stimulus of moderate intensity. Under these conditions the *O*s got memory after-images in the auditory field which seem to be like those which Fechner describes in the visual field; that is, they were good copies of the stimulus and were of short duration.

Fechner further states that there were individual differences in the appearance and characteristics of the memory after-image. For some they closely resembled the after-image, while for others they were more like the memory-image. We found similar individual differences in this investigation. For Su, De, and St the memory after-images were sensory in character; for F they were more like the memory-image; while for Di they partook of the characteristics of both.

To further substantiate the uniqueness of the memory after-image experience, Fechner describes four conditions which set it off from the after-image on the one hand and the memory-image on the other. These criteria of differentiation were naturally of interest to us, but since we have worked in the auditory field where an after-image corresponding to the visual after-image is lacking, we have not been able to make any complete verification of these criteria. Differences in method due to differences between visual and auditory experiences have fur-

[5] G. T. Fechner, *Elemente der Psychophysik*, 1860, II, 491 ff.

ther rendered a detailed comparison impossible. Some of the criteria, however, have been general enough to warrant a comparison of our results with those of Fechner.

The first criterion[6] states that, while after-images exist whether one has attended to the stimulus or not, attention to the stimulus is necessary for a memory after-image as for a memory-image. It is to be noted that the degree of attention, whether focal or marginal, is not stated, though Fechner says that the sharper the attention to the stimulus the clearer the memory after-image. Exner,[7] who speaks of "das primäre Gedächtnissbild" but who is evidently referring to the same phenomenon as Fechner's "Erinnerungsnachbild," says that it appears whether attention has been turned to the preceding sensory impression or not. He cites as an example the fact that people otherwise attentively engaged sometimes do not hear the first strokes of the clock striking the hour. They are able, however, to count them from the beginning after a number have gone by. This is done through the use of "das primäre Gedächtnissbild." Here, then, is a disagreement as to the conditions under which the memory after-image appears. Daniels,[8] who refers to both Fechner and Exner, carried on an experiment in which the *Os* read "interesting stories in a loud voice and with the greatest possible rapidity. The image used was that aroused by the pronunciation of three digits. When the subject was well under way in his reading, the digits were announced by the operator, the three occupying about two seconds. After a determined interval, at a signal from the operator the subject ceased reading and endeavored to recall the digits that had been given." The purpose of the experiment was to determine the duration of the memory after-image. The thing in which we are interested, however, is the fact that the subjects could recall the digits after the interval, though there was distraction of the attention during the original presentation of them. Daniels states that "absolutely perfect distraction was not secured," but it is certain that a high degree of attention was not given to the sound of the digits. Daniels raises no question as to the nature of the memory after-image, but assumes that the digits were recalled by means of it. His results, so far as attention to the stimulus is concerned, agree with Exner's, not with Fechner's, description of conditions necessary to the arousal of the memory after-image.

In order to secure an answer for ourselves, if possible, the following instructions were given to the *Os*: "I shall give you some figures to add. Add them as quickly and as accurately as you can. While you are adding I shall use as a distraction a number of taps given in series. Try not to be distracted by them. When I say 'Now' stop adding immediately." *E* gave a series of taps in groups ranging from one to six in number. When *O* seemed engrossed in the task of adding, *E* said "Now" and asked: "How many times did I tap last time?" The results obtained from the first trial (the most significant, because the *Os* did not know that they were to be asked to report on the number of taps and so had no reason to attend to their number as was the case in later trials) follow. Su gave the right answer and said: "I can still hear the last group in imagery and count from that. I was not counting the groups as you tapped." De gave the right answer and reported that he was not counting the taps during the addition. He did not know how he knew the answer. Later he reported that as soon as *E* said "Now" the taps "seemed to come to consciousness." F did not give the right answer. He said: "I wasn't counting taps until you spoke but then I counted them in memory. I counted the auditory repro-

[6] *Ibid.*, 493.

[7] S. Exner, Hermann's *Handb. d. Physiol.*, II, 1879, 281f.

[8] A. H. Daniels, this JOURNAL, 6, 1895, 558ff.

duction in verbal-motor imagery." St obeyed the instructions not to be distracted so well that at no time could she tell the number of taps. She reported: "I have no idea how many times you tapped. . . . It was just a racket, not groups of taps at all." Further observations will not be quoted. They substantiate the first, but the conditions were not so good, since the *Os* now knew that they would be asked to tell the number of taps and tended to give some attention to them; so that we were never sure that the taps were not counted as they were given. We have, however, evidence that a high degree of attention is not necessary for the arousal of the memory after-image, though it seems probable that some attention is necessary since St, and some of the other *Os* at times, attended so well to the adding that they gave no attention to the tapping and as a result had no idea of the number of taps in the last group. We have pointed out that we do not know to what degree of attention Fechner refers. If for him focal attention is a condition of the appearance of the memory after-image our results are not in accord with his. On the other hand we cannot agree with Exner[9] (within the limits of our experiment) that "this primary memory image is, whether attention has been turned to the impression or not, an extremely lively one."

Fechner's second criterion states that one must take the attitude of actively calling up the memory after-image as one does the memory-image. It will not come with the purely receptive attitude which suffices for the ordinary after-image. Our results do not fully agree with this condition. It may be remembered that all of the *Os* reported the occurrence of the memory after-image spontaneously. It is true, of course, that they were "set" from the first to observe any mental processes present during the specified interval. That they were not, however, actively searching for an image is evidenced in the following reports. "The image seems to happen. I sit here perfectly helpless and it just comes" (Su). "The last word of a group of words seems to stick in my mind and to reëcho at the same time that you're talking" (De). "When I stopped the rest of the sentence sounded vividly in mind" (F). "I could hear the last three words just as you said 'Stop' repeated in auditory imagery" (Di). "I'm not making any effort. I hear the words after you've stopped" (St). In later reports on the memory-image Su said: "I can stop this image (memory-image) anywhere and have it go on again, but when I am taking dictation (memory after-image) I can't; I usually can't help myself but listen to it finish." De reported that "these images (memory after-images) seem to persist from the perception. I can get them without verbal-motor imagery which I have to use to get the memory-image." The same sort of experience occurred in the case of Di. For F the memory-images were always "voluntary" while the memory after-images were not. We cannot say, of course, that our *Os* were purely receptive, since they were set to observe the auditory memory after-image, but all of them had actively to try to get a memory-image (as Fechner asserts), whereas the memory after-image came with only the intention to observe it. Considering the memory after-image from the point of view of our results with respect to the above criteria it seems to be more like the after-image (if there were one in audition) than the memory-image. But we have pointed out that Fechner says that for many people the "Erinnerungsnachbild" seems like the after-image. His criteria of differentiation seem to have been based on his own experience of the memory after-image, which was for him more like the memory-image.

The two remaining criteria deal with the difference between the after-image and the memory after-image and are applicable only to visual phenomena.

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[9]Exner, *op. cit.*, quoted by James, *Prin. of Psychol.*, I, 1905, 646.

There is one more aspect of the memory after-image described by Fechner with which we have concerned ourselves. He says that, when the memory after-image was dying out or losing its clearness, he could often call it back by "eine neue Anstrengung".[10] This revived image was less clear than the original. We asked our *O*s to call back the auditory image after the memory after-image had gone. This was done by all *O*s only with effort. For all *O*s the revived image was initiated or accompanied by some other kind of imagery, most frequently verbal-motor, though with some *O*s visual. The auditory component was described by one *O* as "only over-lying" the verbal-motor imagery, and this was the kind of experience that all *O*s seem to have had with the possible exception of Su who, after initiating the auditory imagery by means of verbal-motor imagery, sometimes got the auditory component to "go along alone." These revived images were for all *O*s weaker in intensity than the perception. For three *O*s (Su, De, St) the duration was the same, though for Di the image "dragged," and for F the duration was as long as he "wanted to make it." In all cases the pitch was the same as that of the perception. The richness was less for De, Di, and F, and the same as that of the perception for Su and St. Our *O*s could not, then, call back an image at all comparable with the first or true memory after-image. The recurrence always took on the full characteristics of a memory-image. There is no justification for calling it a memory after-image. Fechner himself noted that the revived images were always weaker and less distinct. Without doubt the fact that we have worked in the auditory field has put a premium on the use of verbal-motor imagery, for which there would be much less, if any, use under similar conditions in the visual field. It is interesting to observe, however, that all *O*s got excellent auditory memory after-images with rare reports of any verbal-motor accompaniment, while none got auditory memory-images without the aid of verbal-motor imagery.

L. J. Martin[11] has also described experiments dealing with the memory after-image. She was interested in the relation which exists between the memory-images described by her with the use of the projection method and Fechner's "Erinnerungsnachbild." She, too, worked in the visual field. A sample report from one of her *O*s will indicate the nature of her results.

"When I looked out of the window and closed my eyes, a light flashed before me brighter than the light outside. In this I saw only the tops of certain trees in the scene. They are of a bright color in reality, but the flash of light made them even brighter. This flash was instantaneous, and as it disappeared I became aware of the memory-image of the scene, which

[10]Fechner, *op. cit.*, 493.

[11]*Op. cit.*, 358f.

I feel I have had all the time, but which the flash of light had obscured. The memory image was clear, steady and accurate in detail . . . . On closing my eyes later, I could bring back the second image as it was by an effort of will, but the first image I found it impossible to bring back as it was. . . . . "

This and other similar reports lead Martin to conclude that Fechner's memory after-image does not exist as an unique phenomenon, but that it may be considered a combination of the after-image (positive or negative) and the memory-image. "Denn wie die Resultate beweisen, findet sich in Verbindung mit visuellen Vorstellungsbildern meist nichts, was selbst Fechner als ein Erinnerungsnachbild bezeichnen könnte, wo aber derartiges zu beobachten ist, da erscheint es keineswegs als psychologische Einheit, sondern als ein Zusammenwirken von Spuren, die ihrerseits zwei verschiedene Arten von Bildern: Nachbild und Gedächtnisbild unterscheiden lassen."[12]

If, however, there is no positive after-image in audition analogous to that in vision, then, according to Martin's view of the memory after-image, we have not been dealing in this experiment with memory after-images at all. Yet we have been concerned with experiences very like those described by Fechner, Exner, and Daniels. In order to determine whether there was in the auditory field anything analogous to the disappearance of one image and the appearance or awareness of another, which led Martin to make her analysis, we asked our *O*s "to report on all that occurs from the time I say 'Stop' until I say 'Now.' " The interval was 10 sec. The same general procedure as used before was followed here. No one of our *O*s found that an auditory memory-image came spontaneously during the interval. The auditory memory after-image described here by all *O*s came, disappeared, and was followed by a blank interval.

St said: "The memory after-image finishes out. Then I hear the 'Stop' and then I think mostly of when you are going to say 'Now'." F said: "I waited to see if the after-image would be repeated. It wasn't." De reported that there was nothing after the after-image disappeared and he "wondered what to do to fill in the time." Su got a second image, but said: "I think I call the images back after the first time. I have to remember that they are in your voice and I go along with them in verbal-motor imagery. There seems to be more effort." Martin's *O* reports that "both images came wholly involuntarily on closing the eyes."[13] Di also reported a second image, but like that of Su his was verbal-motor.

We have, then, nothing like Martin's two stages, granting even the possibility of a positive after-image. The image for our *O*s came and died out as we have shown in our description of its temporal course. After an interval, for two *O*s a second image came, but it was not there "all the time", and it did not

[12] *Ibid.*, 367.
[13] *Ibid.*, 359.

come "wholly involuntarily." Certainly the memory after-image in audition does not submit to Martin's analysis. We must bear in mind, of course, the fact that she was writing only of visual images and indeed says: "Ueberdies besitzen wir noch keine experimentellen Daten, welche die Annahme, dass auch bei anderen als visuellen Vorstellungen ähnliche Phänomene auftreten, wahrscheinlich machten." [14]

Conclusions

Our results show that in the auditory memory after-image we are dealing with a psychologically unique experience. We have assumed that there is no positive after-image in audition which would account for the experiences we have described. We have found that the auditory memory after-image differs considerably from the memory-image. It seems to partake of the characteristics of both the after-image (in vision) and the memory-image. Like an after-image (in vision) it is "just there," "persistent," and is localized outside of the head; like a memory-image it is less rich than the perception and for some *Os* less intense, though there are individual differences. Martin's analysis would in no case seem applicable to the auditory memory after-image, for we did not find a blending of after-image and memory-image, though we did have evidence of a memory-image coming *after an interval*, voluntarily, and in a different sense-department. If the "Erinnerungsnachbild" is to be denied existence as a "psychologische Einheit," our results seem to show that this can be done only by way of an auditory positive after-image (which the auditory memory after-image studied here closely resembles), and there is so far no evidence for such an after-image in audition.

In any classification of images the memory after-image belongs where its name implies, between the after-image and the memory-image. Whether it should be placed nearer the after-image or nearer the memory-image depends upon differences between the individual *Os*. This dependence upon individual *Os* seems to make it more like the memory-image, but its many sensory characteristics put it in the after-image group, so that the name "Erinnerungsnachbild" or memory after-image seems well chosen.

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[14] *Ibid.*, 367.

A PECULIAR TRANSFORMATION OF PERSONALITY DUE TO *ENCEPHALITIS LETHARGICA*[1]

By Donald McNeil

Transformations of personality are always interesting. A number have been noted due to psychogenic factors, hysteria, etc. The transformation of personality that comes after an epileptic seizure and lasts for a few minutes or a few days is a well known phenomenon. A transformation of personality due to a specific disease of the grey matter of the brain, and therefore clearly based upon organic factors, is such an interesting phenomenon that it is well worth studying in detail and reporting. It was thought, therefore, that the meager information we first had about the change of disposition manifested by one of our cases in the clinic was worth more thorough investigation. The resulting study seems so interesting that it is determined to publish the record.

To bring out the items of this transformation more clearly, we thought it advisable to make use of some definite method. The method followed in this study was that suggested by F. L. Wells, described in the following paragraph, and modified to meet the special needs of this experiment.

> "The manner of presentation is to enumerate under the title of each division, first, the topics that are arranged under it to form a part of the present system. They are not formulated in the simple names of qualities, as in the schemata of Cattell or Davenport, but in the form of questions, similar to the Heymans-Wiersma and the Hoch-Amsden; *e.g.*, how well does he keep his word, how sociable is he. This often gives us a sharper definition than is possible through single names of qualities. . . . . Immediately following these are quoted under each section the topics that best fit under it from (1) the revised scheme of Hoch-Amsden (those of a supplementary nature only), (2) the list of Heymans and Wiersma, (3) the series arranged by Professor Cattell, (4) selections from the 'Trait-Book' of Davenport . . . There is added a brief discussion of each division, more closely defining the topics of the present schema, and the relation of each section to the others and the entire series." [2]

The method of notation we have used is similar to that recommended by Wells. The excess of the attribute (that is, above the average) is indicated by an asterisk, a deficiency by a minus-sign. Where the excess or deficiency is very prominent, an exclamation mark follows the asterisk or the minus-sign; and

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[1] From the Psychological Laboratory of The Catholic University of America.

[2] *Psychological Review*, 1914, 21, 298-299.

where the excess or deficiency is but little above the average, or where there is some doubt, a question mark follows the sign. These signs, as will be noted, have been grouped under the three headings, BI, AI, and AT, meaning respectively before illness, after illness and after treatment.

The treatment herein referred to consisted in a low protein diet with intramuscular injections of tethelin, a drug supposed to contain the active principle of the anterior lobe of the pituitary gland.[8] Though we noted the condition of the patient both after his illness and after treatment, we are by no means positive that the improvement noted was due to the treatment rather than the natural healing process of time. It would seem, however, that the patient acquired better control after a relatively short stay in the hospital; and although his friends from the union thought originally that he should be sent again to an asylum, after his stay in the hospital he was able to go back to work and resume his trade as a plumber.

After each question concerning the trait we have not only indicated its presence or absence in excess or defect, but have also attempted by brief remarks placed at the right to indicate little incidents which lead us to believe that the trait in question was present or absent.

Owing to the extreme difficulty of obtaining satisfactory answers to all the questions suggested by Wells, some of them have been purposely omitted. In general, however, the questions employed are in close conformity with the arrangements suggested by his scheme.

History of the Case

The subject of this study was born October 15, 1896. As will be seen by consulting the tables, the patient, when a child, was of a quiet, reserved disposition. He did not indulge in games or athletic sports to the extent that the average boy does. Probably one of the reasons for this was the fact that he was flatfooted and afflicted with a certain stiffness of the knees which interfered, to some extent, with his motor activities.

On February 15, 1921, the patient was admitted to one of the city hospitals suffering from a condition later diagnosed as *encephalitis lethargica*. The illness started with a pain in the right side of the abdomen and disappeared in a short time. It reappeared, however, on the left side and finally became centered in the head. A week later the patient saw double and began to get drowsy. During his sleep he was very noisy, shrieking and crying out as if in great distress. For the first five days of his stay at the hospital he experienced frontal headaches for about two hours a day. During these attacks he had strabismus, the eyes slowly regaining their normal position after the attack.

After twenty days of almost continuous sleeping the drowsiness began to disappear, and a short time afterwards the patient was discharged from the hospital.

A decided change was then noted in the disposition of the patient. His manner and outlook on life underwent a complete transformation. In

[8] T. B. Robertson, *Journal of Biological Chemistry*, 1916, 24, 409.

spite of loss of appetite, the patient gained thirty-two pounds in four weeks, this being followed by a short period of no gain. This was followed by another increase of thirty-three pounds in the next five weeks.

Several months later the patient was admitted to a sanitarium for mental disorders where he remained for a few weeks, finally escaping and returning home.

He first visited Providence Hospital Dispensary on January 4, 1922. The following were his complaints: vomiting, loss of sexual power, abnormal increase in weight, indigestion and a tendency to fall asleep. The patient came to the clinic accompanied by two members of his trade union, who told me that although previously he had been efficient, after his illness he was discharged from several jobs, sometimes for flirting with girls in the building, sometimes because he had bungled his work, ruined costly material, etc.

An x-ray examination revealed the following: sella turcica small and of the closed-in type, with no evidence of bone disease. There were definite shadows in the sellar space, due probably to calcification of the gland.

The family history, outside of the fact that the father was alcoholic, was negative.

Diagnosis

The case was diagnosed as *dystrophia adiposa genitalis*. This was probably the result of the previous *encephalitis lethargica*.

At the direction of Dr. T. V. Moore, of the staff of Providence Hospital, the patient was admitted to that institution to undergo treatment and to furnish an opportunity to make a thorough examination of his case both mental and physical.

1. Intelligence

Question	BI	AI	AT	*Remarks*
How easily does he learn?	?*	?*	?*	Patient when a boy was a better mathematician than the average of his class.
Is he a good observer?	?*	?*	?*	
What fund of information relative to educational opportunities?	!.	*!	*!	An oral examination was given the patient in general information. The knowledge he had of geographical and historical facts was astounding, considering his limited school experience. He was also conversant with current events. An examination after cessation of treatment by the army trade-tests for plumber, gave him the rating of a middle-grade apprentice.
How good a memory?	*	*	*	Remembers minutiae in events of which the average person has no recollection.

Is his advice sought by others?				Never was in a position where anyone would seek his advice.
Does he plan with good foresight	–?	–?	–?	
2. Output of Energy				
How much motor activity?	–	–	*?	Flat feet and stiffness of the knees hindered his movements when a boy. Since treatment he seems to move around more than usual.
How talkative?	–!	*!	*!	As a boy the patient was more reserved and less talkative than the average boy. Now he talks incessantly when he has someone to listen to him. Friends of his have heard him talk vehemently when alone as if disputing with someone.
How conventional?	*!	–!	–	Since his illness he does the most unconventional things. On one occasion he knelt before a young woman and with affectionate gestures asked her to accompany him to the theatre.
How skilful with tools?	*	–	*?	Before his illness he was a very skilful plumber. After his illness he bungled several jobs and was finally dismissed. After being treated at the hospital for a while, he performed several small jobs successfully.
What degree of bodily dexterity and grace?	–	–	–	The patient was always awkward in his movements and since his illness this awkwardness has been increased remarkably.
3. Self Assertion				
How much effort to shape surroundings?	–	*?	*?	Used to assume a passive attitude. Is now more critical.
How independent of the opinions of others?	–	*	*	Patient is not susceptible to advice now, even to such as is to his best interests.
How much tendency to assume leadership?	–	–	–	Never has shown any such tendency.
How ambitious in material things?	*?	–	–?	Ambitious to save money before illness. Now he spends extravagantly.

How able to bear up under difficulties and misfortunes?	* *? *?	After illness he felt sure there was something wrong in the attitude of his friends. His excessive talking may be an outcome of this. He thinks that it will cover up his deficiencies.
How able to face a crisis?	*?	Before illness he had fair presence of mind; since his illness no opportunity has presented itself to note how he would behave in a crisis.
What inclination to face danger?	– – –	Has always kept away from sports and amusements which were dangerous.

4. Adaptability

When a child how did he get along with other children?	*	Very quiet. Not aggressive. Was fairly well liked but never a leader in boyish sports.
How does he get along with older people?	* –! –	Was very good to his parents before illness and was liked by older people. Since his illness, however, his parents have trouble with him.
Is he tactful?	* – –	Now has absolutely no tact.
Is he offensive?	– *! *!	Before illness he was most inoffensive; since his illness he is the reverse. Now he is continually intruding where he is not wanted and says things that give offence though he does not mean to be offensive. He makes advances to every girl or woman that he meets. On one occasion he was arrested for annoying a girl on the street.
Is he quarrelsome?	– – –	Never had a fight in his life.
What tendency to be guided by advice?	*? –! –!	In the hospital he has to be watched carefully to see that he carries out the advice of his physicians.
How resourceful? Does he adapt himself to new environments?	–? * *	Is quite at home in the hospital; he does not seem to care particularly where he is situated.

5. Moral Sphere

How well does he keep a given word?	* –! –!	After his illness he promises anything with no intention of keeping his promise.

How truthful in matters relating to present or past?	*	−!	−!	After illness he told his parents stories about people which were not true. Now they cannot rely on his word at all.
Is he trustworthy in money matters?	*	*?	*?	As far as can be ascertained he still seems to be fairly honest.
Is he frank with opinions?	−?	*!	*!	Since his illness he is decidedly so. He is not the least bit backward in telling anyone what he thinks of them.
How conscientious in the performance of duty?	*!	−	−	Before illness he always told employer if he broke anything, but after illness tried to conceal these facts.
How discreetly careful of the reputation of others?	*	−	−	Not at all careful now. Says things about people that are not true.
How mindful of the equal rights of others?	*	−	−	On one occasion he fell asleep in a theatre with his head resting on a young lady's shoulder. This occurred after his illness. Because he was ejected for this action he wanted to sue the management, asserting that they had no right to complain of his conduct.

6. General Habits of Work

How prompt in reactions to situations?	*?			No situations have arisen in which such reactions could be noted.
How systematic in work?	*	−!	−?	Was very systematic in work before illness; after illness would start a new job before the old one was finished. Shows improvement since treatment.
How persistent?	−?	*!	*!	As a boy when he wanted anything, and was told that he could not have it, he would forget about it; in the hospital, when he is told not to do a thing, he persists in asking permission to do it.
How punctual?	*!	−!	−?	After illness he would arrive late for work, would sleep on the job, etc. He was finally dismissed from service.

7. Recreative Activities

Sports requiring quick and continuous action.	–	–	–	
Less active sports.	–?	*?	*?	Likes to take long walks now.
Hunting and fishing.	–	–	–	
Camp life.	–	–	–	
Games of intellectual character.	–	–	–	
Gambling.	*!	*?	*?	Before illness was a heavy gambler and was fairly lucky. Since illness has not gambled very much, owing more to the lack of money than anything else.
Alcohol.	–!	–!	–!	Has never taken a drink of any intoxicant.
Tobacco.	*?	*!	*!	Smokes cigarettes; several packages a day since illness.
Other drugs.	–!	–!	–!	
Reading.	*?	*?	*?	Reads some light literature.
Music.	*?	*?	*?	Enjoys listening to popular music. Does not play any musical instrument.
Pictures (moving).	*	*!	*	
Artistic creations.	–	–	–	
Delicacies in eating and drinking.	*	*!	*!	Very fond of ice-cream. After illness used to eat ten plates a day.
Sports involving physical danger.	–	–	–	Has never indulged in these sports, because he was afraid that he might get injured.

8. General Cast of Mood

Is he cheerful?	–	*?	*	At first the patient seemed despondent but now he is extremely cheerful. Although he is anxious to get better his illness does not make him despondent.
Does he get despondent without apparent reason?	*?	–	–	
Does he enjoy his discomforts?	–	–	–	

Has his mood been changed by any occurrence or circumstance?				His mood has been changed completely in many respects by his illness.
Did he have tantrums as a child?	*?			
Is he easily consoled?	*	*?	*?	Requires greater consolations now than formerly.

9. ATTITUDE TOWARDS SELF

Is he self-conscious?	*!	–!	–	Before illness was very self-conscious. After illness casually addressed three hundred people at a union meeting.
Is he conceited?	–!	*!	*!	The patient now has the idea that every girl should fall in love with him.
Is he self-satisfied?	*?	–?	–?	Patient wants to lose weight. He thinks his appearance will improve with loss of weight.
Is he concerned for the present or future?	*	*?	*?	
Is he inclined to pay much attention to his aches and pains?	–!	*!	*!	He wants to tell everyone the history of his illness. Before his illness he would never complain, no matter how grievous his affliction.
Is he patient?	*	*?	*?	Becomes impatient with his mother when she does not have his meals ready when he wants them.
Does he talk of self?	–	*!	*!	Will talk about himself all day if anyone will listen to him.
Is he sympathetic?	*!	*?	*?	Used to be very sympathetic before illness. Now the question of self seems to interfere with his being sympathetic.

10. ATTITUDE TOWARDS OTHERS

Is he generous?	*!	*	*	Asked cousin who visited him at hospital for money. When she admitted not having any, he wanted to give her what little he had. Was the support of his parents before illness.
Is he jealous?	–	–	–	
Is he sensitive?	*!	–	–	It was very easy to hurt his feelings before his illness. Nothing seems to hurt them now.

Is he forgiving?	–?	*	*	Forgave his girl for going back on him even though he knew she thought him crazy.
Is he grateful?	*!	*?	*?	At times he shows gratitude, other times the very opposite.
Is he considerate?	*!	–	–	Before illness would never say anything that might cause his mother to worry; now he does not seem to consider her feelings at all.
Is he selfish?	–	–?	–?	Now has a greater desire for his own comfort to the discomfort of someone else.
Does he keep his friends long?	*!	–?	–?	Had friends for over fifteen years. But since his illness the majority of them will have nothing to do with him.
Is he fault-finding?	–	–?	–?	When he goes home from the hospital for a visit he complains of everything which does not meet with his approval.
Is he suspicious?	–?	*!	*	Since illness is rather suspicious. Suspects people of making fun of him; also thinks that certain persons are plotting against him.

11. Reaction to Attitude Towards Self and Others

How scrupulous of personal appearance?	*	–	–	Was an extremely neat dresser; now, especially around hospital, does not care in the least about his appearance.
How sociable?	–	*!	*!	Will make friends with anyone he can.
How socially forward?	–!	*!	*!	Since illness thrusts himself upon people and does not realize when he is not wanted.
How demonstrative of emotion?	*	*!	*!	Likes to fondle children, especially since his illness. Will demonstrate affection for anyone who will let him.
What tendency to unburden?	–!	*!	*!	Patient after illness would tell absolute strangers all about his troubles.
How great a demand for sympathy?	–	*	*	Wants to have people sorry for him.

How much pleasure in the enjoyment of success of others?	*?	*?	*?	
How much given to witticisms?	–	–	–	
How even-natured?	*	*	*	Never seems to get angry or become nervously excited.
Has he a different manner towards superiors and inferiors?	*	*	*	Is very respectful to superiors, but tries to boss those who he thinks are under him. Likes to talk in a loud commanding tone to the elevator boy at the hospital.
Is he more frank to certain people.	*?	*	*	Will admit certain things to one person which he will absolutely deny to another.
Is he open?	–!	*	*	Will tell the most intimate things about himself with the utmost unconcern.
Is he secret?	*!	–	–	
Is he forward?	–!	*!	*!	Very forward now; will make advances to girls on the street or elsewhere, even though he does not know them. Nurses at the hospital had much trouble with him on this account.
Is he bashful?	*?	–!	–!	Will do and say things since his sickness that he wouldn't even think of before.
Is he dull?	–	*?	*?	

12. POSITION TOWARD REALITY

What capacity to take things as they are?	*	*	*	
Is he practical?	*	–	–?	Patient wants to get married although he is not working and has no prospects of a job.
What capacity to acknowledge mistakes or transgressions?	*	–	–	Patient is on a diet at the hosppital, but ate outside at other places. When questioned he denied having eaten anything he was forbidden to eat, but finally admitted that he had.
Is there any tendency towards day-dreaming?	*?	*?	*?	Given to imagining how things would be if he were married and had a home of his own.

How influenced in actions by likes and dislikes?	–	*	*	

13. Sexual Sphere

How forward toward the opposite sex?	–!	*!	*!	Before illness went with but one girl and wanted to marry her; now he wants to make love to every girl he sees.
Is his personal attitude in harmony with his own sex?	*	*	*	Patient is not effeminate.
Is he attracted by older persons of the opposite sex?	–	*!	*!	
When love affair was broken off what was the reason?				Had only one love affair. This was broken off by the young lady because she thought him insane.
Was he decided when question of marriage came up?	*	*	*	Patient was and is still anxious to get married.
Is there a desire for children?	*?	*!	*!	Desire for children is very strong especially since patient knows that the likelihood of his having any is very doubtful.
Is he flirtatious?	–?	*!	*!	
Does he have sexual trends in conversation, art, etc.?	–?	*?	*?	Books and magazines containing questionable stories have a certain amount of attraction for him.
Did he ever practise masturbation?	–	*?	*?	Patient denies the practice but admits having allowed "tactus ex parte puellarum."
Is he prudish?	–	–	–	
Is he content with existing sexual adjustments?	*!	–!	–!	Does not understand why the opposite sex should not be strongly attracted to him. Does not wish to stay single.

14. Balancing Factors

Is he firm in religious belief?	*	*	*	Patient seems to live up to his religious beliefs.
Is he active in church work?	*	–	–	
Is he superstitious?	–	–	–	

Is he religious?	*	*	*	
Is his conduct consistent?	*	*?	*?	He does things which formerly he would think not just the proper thing to do.
Has he great schemes?	–	–	–	

Summary

From a study of the above tables we may make the following summary.

(1) *Intelligence.* As the study revealed, the patient, considering his educational opportunities, was of more than ordinary intelligence. Although dull and stupid during, and for a time after, the attack of *encephalitis lethargica*, he seems to have recovered to a large extent. A number of questions intended to test his knowledge of current events was asked him, his answers exhibiting a fair amount of information about the recent war, politics, etc. The uniform ability he showed throughout the test to deal with questions of this character seems to justify the conclusion that his illness has not left his intelligence seriously impaired.

(2) *Output of Energy.* As a boy, the patient showed but slight inclination to exert himself physically. Unlike most children of the adolescent and pre-adolescent period, he indulged to but a very small extent in the ordinary activities peculiar to that age. This was probably due to the physical defects from which he suffered (flat feet and stiffness of the knees), although he did not even participate in such activities as fishing, camping, hunting, etc., which are usually a part of every normal boy's life. This peculiarity is all the more noticeable since the patient lived near a large river that afforded good opportunities for such sports.

In his youth, the patient was more than normally quiet; now he is extremely talkative. This change, which he himself appreciates, is one of the most profound noted. However, he seems to be improving with the tethelin treatment; but this may be due, of course, to the change in environment.

Though he was discharged from several jobs after his illness because of inefficiency, he is now so far improved that recently he was able to perform several pieces of work for the hospital authorities with success. He has since been working regularly at his trade of plumbing. The probabilities, therefore, are that his dexterity as a workman, which suffered deterioration during his illness, may eventually be fully restored.

(3) *Self Assertion.* Under this head will be noted a very profound change; it represents a radical change of attitude and disposition on the part of the patient from that exhibited previous to his illness.

Before his *encephalitis lethargica* the subject was remarkably quiet, thoughtful and thrifty. These were leading traits in his character, his shyness and bashfulness being especially pronounced. Since his illness the patient seems to have assumed a new personality. Now he is very loquacious, heedless and extravagant. One explanation for his excessive talkativeness might be found in his desire to obscure and forget his past difficulties and misfortune. It might be due also to a new confidence that the patient has in himself. On the other hand, though he talks a great deal about what he is going to do, he remained for some time very inert. While at the hospital he made but feeble efforts to change his surroundings or improve his condition. In this respect he is very careless. Since his illness, however, he is very self-assertive and tenacious in his views. These fundamental changes in his disposition are especially apparent to friends intimately acquainted with him before his illness. Many of them on this account do not esteem him as much now as formerly, and for the same reason he has lost the regard of many of them. At the present time, however, he gives a slight indication of becoming his former self.

(4) *Adaptability.* Previous to his illness, the patient, because of his mildness and thoughtfulness, was liked by young and old. Now, however, owing to his change of disposition, he is generally disliked or simply tolerated by older people with whom he comes in contact. The chief traits in his changed manner that seem to be responsible for this circumstance are lack of tact and undue inclination to talk about his own ailments. While he is not quarrelsome, he will nevertheless adhere stubbornly to his own opinions, not hesitating even to argue with his superiors on matters with which he has no concern. Since beginning the tethelin treatment he seems to have improved somewhat in this respect, and it has been noticed that at least two patients of his ward show a liking for him.

(5) *Moral Sphere.* Several marked changes are noticeable in the life of the patient. Before the affliction he was uniformly conscientious, truthful and discreet; after his illness these characteristics were no longer present. Although some improvement in this respect is apparent, the subject still lacks these elements of reliability which he exhibited formerly. It is possible, however, that some recovery in this direction may take place.

(6) *General Habits of Work.* A marked effect of the patient's illness was a radical change in his habits of work. Prior to his illness, the subject was a competent, thorough and reliable workman. After his recovery he no longer possessed the same punctuality and systematic habits of procedure. He became very untrustworthy and would abandon a job before its com-

pletion to begin something new. He would frequently be late for work and, when the opportunity came, would sleep during working hours. For these things he was finally dismissed from the service of his employer. A change for the better appeared after a course of treatment with tethelin. The patient was given several jobs to do at the hospital, all of which he completed very satisfactorily, and is now employed regularly at his trade of plumbing.

(7) *Recreative Activities.* Under this heading several marked changes are noted. A slight change toward more active interest in the less active sports appears. This may be due to the fact that the patient has been confined for some time; and consequently the less active sports, such as automobile driving and walking, appeal to him more than they otherwise would.

A slight increase in the number of cigarettes smoked per day is also noted. This may be accounted for by the fact that a person when occupied will not smoke as much as when idle; the patient being idle will naturally smoke more than was his custom.

The abnormal fondness for ice-cream is a probable result of the disease. He was always very temperate in his habits and tastes. He never used alcohol or other drugs, and for diversion has shown a preference for games of the less intellectual character.

(8) *General Cast of Mood.* A change in the mood of the patient due to his illness is very discernible, although the variation of this trait from the normal is not so decided as it is for some of the others studied.

So far as can be ascertained, the patient during his boyhood and youth was very quiet and moody, bordering even on the melancholic. He rarely showed any exuberance of spirit. One would expect, therefore, that under the present circumstances this condition would manifest itself in more aggravated form. Nevertheless, in his whole illness he has never been despondent. He takes things lightly and shows no disposition to brood over his difficulties.

This peculiarity, exhibited by the subject, of aspiring to situations beyond his means of realization would be interpreted by some psychologists as indicating a disordered personality. In an article contributed to the *Psychological Review* by a student of psychology in 1914, the following conditions are laid down as the essentials of a perfectly balanced personality:

"Among the essentials of a perfectly balanced personality are that its acts shall accord with experience rather than with a contrary feeling, that it shall react to situations objectively rather than with a contrary feeling, that its aspirations shall be within and in accord with its capabilities, that its power of judgment shall not be impaired in crises of danger, or the incidence of good or evil fortune, and that there shall be no conflict between

morals and conduct. . . . . In the ill balanced personality we find essential disharmonies between the affective reactions and those of the proper objective value."

Applying this norm to the case in hand, it is apparent that the aspirations are not in accord with the capabilities: the patient is desirous of getting married when his condition precludes the possibility of his doing so; his power of judgment is defective; and, considering his moral ideals, his conduct on some occasions would seem to contradict his better moral views. Accepting the principle enunciated above as correct, we are led to the conclusion that the personality of the patient is ill-balanced. Further force is lent to this view by the fact that the patient, though he realizes his condition, shows no tendency to be depressed or despondent.

(9) *Attitude towards Self.* The effect of the illness on the patient is conspicuously shown by his changed attitude towards himself. Before he became sick, the subject was unassuming, modest and very reticent in matters relating to himself. In personal matters he would confide but little even to his parents. Now he seems to take a delight in telling everyone about his sickness and about his broken love-affair. Since his illness he is known to have addressed a large audience apparently without experiencing any embarrassment. Such action is entirely out of keeping with his former conduct. These instances illustrate the breaking down of former inhibitions. While it might appear that his abnormal actions are due to an excessive degree of self-confidence, it seems more correct to attribute them to an immoderate amount of self-esteem or an enhanced opinion of his own capabilities. It is very evident that he thinks he is much more gifted than he really is.

As regards his feelings for other people, the patient is not nearly so sympathetic as he used to be. Whereas before his illness this was a leading trait in his disposition, his attitude now in matters relating to self is one of marked egotism. Likewise his former modesty has given place to extreme forwardness. He thinks that he possesses an attractive personality and that he is looked upon with favor by members of the female sex. He is not so loquacious as he used to be, neither is his inclination to make advances to the opposite sex as noticeable as before treatment.

(10) *Attitude towards Others.* Before his sickness the patient stood high in the estimation of his friends. He was easy to get along with and was considerate and unselfish. He was especially mindful of the feelings of his parents, being careful never to cause his mother any worry or annoyance. But after his illness his actions caused her a great deal of anxiety and distress. A peculiar result of his illness was the idea he conceived

that certain people were conspiring to do him harm. Especially was he obsessed by the belief that those people were trying to bring about an estrangement between himself and his sweetheart and ultimately procure his downfall. This caused him to say things to his friends that were the cause of his losing their friendship. For the same reason he failed to cultivate new associates.

This characteristic attitude of distrust has recently disappeared to some extent. He perceives now that people are trying to aid rather than injure him. He realizes also that he himself was the cause for the disaffection of his lady friend.

As regards the transformation in the personality of the patient, it is accepted as characteristic of an unbalanced personality that it suspects where it should trust and trusts where it should suspect. By the manner in which the patient treats his friends of late it might be inferred that a better adjustment in this respect is gradually taking place. The patient now is more prudent in placing trust where it should be placed, and his suspicions are not so unduly acute.

(11) *Reaction to Attitude toward Self and Others.* In this division of the study many changes in the mental life of the subject were revealed. From being bashful and altogether undemonstrative before his illness the patient developed opposite traits equally conspicuous. Scrupulous as he was of personal appearance prior to his illness, he became lax in the matter after his recovery. Recently, however, he has corrected this abnormality to some extent. He gives evidence now of a desire to look well, this being shown by his anxiety to reduce in weight. He thinks that a slight figure will add to his appearance. Likewise, now that he realizes his forwardness, he shows a desire to improve this condition. Although he is still rather forward, a definite improvement is shown. If we interpret this forwardness as being a device employed by the patient to obscure the handicap from which he suffers, we have a means of explaining why this condition has persisted so long.

Although the disposition of the patient has changed in many important respects, basically he is, as formerly, even-natured. He likes to give orders to those whom he thinks inferior to him, showing in this respect a departure from his former non-aggressive attitude, but he does so with no quarrelsome intention.

(12) *Position toward Reality.* Before his sickness the patient was essentially practical. He was a practical man with a practical outlook on life, and his conduct was consistent with his position socially. Morally, he was upright and honest. If he made a mistake he would readily acknowledge it, and he would feel grieved if he transgressed. Neither would he allow his prejudice to affect his better judgment.

His conduct since his illness is not consistent with his earlier record. He is not honest with himself or others and cannot be trusted as before. Though he expresses anxiety to get well, he will not obey instructions if the occasion arises for him to disobey them. At the same time it is difficult to get him to admit that he has broken the regulations. In all there seems to be a certain lack of restraint; for, in some cases at least, he seems to be conscious of the fact that he is doing wrong. In other cases, however, it is evident that his judgment is somewhat impaired. He would, for instance, have married, had his fiancée consented to the ceremony, though at the time he had no means of earning a livelihood and his prospects for the future were poor.

Recently, however, he has developed a more normal condition of mind. One fact that will probably hasten his return to a normal condition is that he is keenly aware of, and sensitive to, his deficiencies. Though he is still unreliable and indiscreet, he seems at the present time to have a will to do what is right.

(13) *Sexual Sphere.* The transformations that occurred in the individual in relation to sexual inclinations are very pronounced. They surpassed all other changes that took place in the intellectual and emotional life.

Prior to his illness, the patient had kept company with but one girl. He loved her and would have married her. In the presence of others of the female sex he was very bashful and ill at ease. With the exception of this one young lady, he was not attracted by members of the opposite sex, and he would never attempt to speak to a young woman to whom he had not been properly introduced. He had no inclination to be flirtatious, differing in this respect from his present abnormal proclivity for females. He has no recollection of ever having practised masturbation as a boy, though he admits that since his illness he has, at least on one occasion, experimented with himself while taking a bath. While he denied secret practices of this kind, he admitted that he had allowed "tactus ex parte puellarum." Since his illness he has been functionally impotent.

(14) *Balancing Factors.* The patient before his illness was very active in church work; and although he does not take a corresponding part in work of this kind since his illness, he is still interested in it. In the matter of attendance at church rites and exercises his conduct is superior to some normal persons. He does not seem to be the least bit superstitious and, so far as can be learned, has never been.

Conclusions from the Study of the Personality

The following is a brief summary of the characteristic changes noted in the patient. They are arranged in two groups. One

group gives the traits that have appeared since the *encephalitis lethargica*; the other, those which have disappeared.

Traits appearing since encephalitis lethargica.	*Traits disappearing since encephalitis lethargica.*
Talkativeness	Conventionality
Frankness with opinions	Skill with tools
Persistency in trying to get what he wants	Ability to get along with older people
Cheerfulness	Tactfulness
Conceitedness	Integrity
Tendency to talk of self	Trustfulness
Sociableness	Truthfulness
Tendency to thrust himself on others	Carefulness for reputation of others
Tendency to unburden	Mindfulness of rights of others
Tendency to talk of his intimate affairs	Systematic method of work
Forwardness	Punctuality
Forwardness towards opposite sex	Tendency to despondency
Interest in opposite sex	Self-consciousness
Flirtatiousness	Sensitiveness
	Considerativeness
	Tendency to keep friends
	Carefulness of personal appearance
	Secretiveness
	Bashfulness
	Contentedness with existing sexual adjustments

Careful study of the above summary gives us the following conclusion concerning the intellectual characteristics of the patient.

There is a paralysis or inhibition brought about by the loss of the power to perceive delicate relations of which the normal man is usually aware, and to take into consideration the possibility of more or less remote consequences.

The increased motor activity may be due also to the breaking down of inhibition, as in the case of acute alcoholism. The cheerfulness may be secondary to this.

Although many character-traits have been noticed as having undergone transformation, it is not impossible that all of these may be reduced to one and the same factor, namely, a paralysis of inhibitions. This paralysis of inhibitions is due to a great extent to the loss of intellectual insights into relations. The patient is not tactful because he does not see the relation of his conduct to ends that would be more readily perceived by a normal individual; he is forward and bold because he has lost due appreciation of the meaning of conduct. Those things that have been built up by education, that act as a restraining influence upon others, have been paralyzed. His behavior resembles very much that of a man slightly under the influence of alcohol. He is clumsy with his tools; he is awkward in his manner; he is talk-

ative, he is cheerful; he has lost all feeling of shame and restraint; he comes late for his job and has no appreciation of what this may mean; he does not care; he has no bashfulness; he has none of the finer sensibilities. The loss of all of these things and the appearance of others does not mean that his *encephalitis lethargica* has produced a change in many attributes of character, but only in one, *i.e.*, control. This control demands, for its perfect exercise, the perfect functioning of a very elaborate cerebral mechanism. It is this cerebral mechanism that has been injured by the *encephalitis lethargica*; and, because of its injury, this peculiar transformation of character has taken place. Such an injury may happen in other ways, and frequently does appear as a transitory disturbance in alcoholism and epilepsy. Unfortunately, with *encephalitis lethargica*, the injury is more permanent. It is not likely that this patient's character will ever again return to what it was before his sickness.

SOME RELIGIOUS CULTS IN JAMAICA

By Martha Warren Beckwith, Vassar College

I. The Revivalists

In the year 1860, a great wave of spiritual excitement swept the island of Jamaica and resulted in a religious revival among the negroes. Its cause no one seems able to explain. Perhaps the impulse came from the United States in response to the high tension of the South on the eve of rebellion. Perhaps the antagonism of the more stable English and Creole population against the methods of the evangelistic orders had at that period reached the fighting point. Its result was the formation of religious groups who gathered about prophetic leaders and organized themselves into bodies of worshippers independent of the established churches on the island. The Revivalists, the Isaiahs, and the Bedwardites are examples of such organizations.

The sight of women going about the streets of Kingston wearing large white turbans, and in the country districts the distant sound of drumming at night were my first intimation of the presence of the Revivalist cult in Jamaica. From outside sources I gained certain facts about these Revivalists. 'They have no church building. They consist in a company of men and women who claim to be inspired by the spirit. The women (called *mesdames*) dress in white and carry wands. They march, sing and pray in the streets at night. They hold their service about a table with a white cloth on which they set a glass of water, flowers and a bible. They rock their bodies in singing and fall down in frenzy. Also they claim to possess cures for driving away sickness caused by the "duppy" (or ghost). They take money from the people for this service.' All this was hearsay; from the people themselves I got nothing. Accordingly, I took some pains to come into touch with individual members of the cult, to attend their meetings, and to learn from their own lips the story of their religious experience.

One night in the little village of Lacovia, in the remote country parish of St. Elizabeth on the south coast of the island, I followed the sound of drums to a revivalist meeting which was held in a booth set up in the bush some distance back from the village street. The worshippers were singing hymns, which began with a Moody and Sankey tune and continued in a dizzying iteration of syncopated rhythm to which they danced with a

peculiar rocking motion, raising first one foot then the other, at the same time lifting the arms and letting the body sway from side to side. Wands held upright in the hand waved also with the beat of the singing.[1] The leader, a man named Granville, wore a uniform resembling that of the Salvation Army and carried a sword-like staff shaped out of wood. Near him stood the leading woman or "Mammy" of the company, a stout, good-looking married woman of thirty-five named Margaret Williston. She wore a white turban and bib-apron and a shoulder-sash of green marked with the figures 66 across the breast. Her wand was made of a lithe vine called "supple-jack," twisted into a ring at the top and tied in a knot. From time to time during the service she spun about slowly or bowed herself. Once, as the singing took on a wilder rhythm, she dropped into a hypnoidal condition which showed itself in a loud rhythmical breathing in sound like the bark of a dog. This is called "trooping" and denotes possession by a spirit. Sometimes it is prolonged until the devotee falls unconscious, but on this occasion Mrs. Williston had herself well in hand and in a few moments was offering a prayer with biblical phraseology. There followed more singing, prayers, testimonies to religious experience. Such a meeting as this, I was told, might last for hours, and the more emotion of the hysterical sort it aroused, the better satisfied were its leaders.

Some days later, Margaret Williston gave me the story of her religious experience, repeating it in a sing-song fashion as if custom had given it rote form. In writing it down here I have not altered her own words, although occasionally their order is changed because some of the facts came out in answer to questions.

I was not a great student. I left school in the first book. And after that I got spiritual at twenty-five. The Mammy was from Westmoreland. When there was shouting, I felt that I was almost shut out of the gate of heaven and I closed my eyes and I said "Lord, if it is thy wish that I be separated, let it be thy will." And I dreamt that I saw an angel and he bids me get a Bible and a Sankey and a rod with a double fold and a lantern which I'm to light on my journey.

I didn't know to pray until I got the Spirit, and the Spirit teaches me to pray and sends me on the high-ways and the hedges to bid others to come and to tell what a sweet Savior I found. Jesus is Savior in need and Savior indeed. There is not a friend like the loving Savior.

The angel which I saw had a bunch of flowers in his hand, white for peace. He bid me get a large turban. He bid me get one larger, but I can't afford it—to make it five yards. He bid me make an apron in white. The sash of green with the number 66 means your shout. You stamp 66 times

[1] H. Junod (*Les chants et les contes des Ba-Ronga de la baie de Delagoa*, Lausanne, 1897, 33) describes the common form of the dance among the Ba-Ronga as performed by soloists and chorus, the chorus forming a circle or half-circle about the soloist and singing the refrain, at the same time holding wands upright in one or both hands which they sway rhythmically to the music while marking time by tapping with the foot upon the ground.

with your foot. Some have a sash of other colors, some a sash without apron. All who carry the wand have a sash, but not all wear the apron. The turban is red or white. The red turban means that you don't feel to work.

Sometimes [at the meetings] as I am in the Spirit moving, the Bible drops and falls open at a chapter and I read and read. I feel to pray and there isn't any chance to pray at the time and I bow to squeeze the prayer-key. When you are in the Spirit, you feel that the world is going around and you turn without you want to turn. When the Spirit overflows within you, you have to shout and shout and shout. You have to shout with joy. When you feel that you can't shout with your voice any more—feel as if it were shut down and locked—then you cease shouting.

Sometimes when a brother or sister is going to depart [die] I feel a fullness within me, I have to speak up, I don't get any ease. But I don't know who it is. If I had the chance to know who the person was, I would go and kneel and make him prepared.

We call our leader "Mammy". She is about forty. She lives in Manchester. But I do not "work" with Mammy, I work for myself. We have not up to now received healing. Once water broke out about five miles away and the people travelled miles. We heal the people according to the Descender. As yet the Descender has not come [to the group at Lacovia] to teach healing.

A few evenings later, in the cock-pit country above Maggotty about fifteen miles from Lacovia, the Revivalists of that district happened to hold a meeting at the turn of the road directly opposite the house where I was staying. I could sit on the upper verandah and from my hill-top catch across the valley the words of exhortation and the reiterated lines of song chanted to the accompanying beat of the drum and the almost incessant bark of the trooping. Afterward, Alexander Edwards, the leader of the company, told me his story. He had been born and reared in the district, like his forbears back through old slave days. A near relative was the leading elder in the Scotch Presbyterian church which he himself attended regularly on Sunday morning. I have no reason to doubt the accuracy of his simple narrative.

I am the leader of the meeting, I and X—because he owns the drum. The leader of the meeting has the dream and he gives it to all the others. My dream was this. I saw angels with these rods in their hands, and they gave one to me. I was lying down in bed sleeping. Often the angel appeared to me. I found myself singing. I waked up and I was fervent in prayer. The song was Sankey, 338,—

> What can wash away my stain?
> Nothing but the blood of Jesus.
> What can make me whole again?
> Nothing but the blood of Jesus.

My spirit felt rejoicing. When the angel appeared, I felt the whole of my body light and rejoicing and I saw the light and from the shining of the light I awoke and began prayer. Everybody heard.

Then [another time] I found myself in school in my sleep and from schooling years I brought examination. And when I went to examination the second time, they told me that I was a student. That year I found examination again and they put a puzzle before me. And after I was able

by the teaching of the Lord to clear it, they told me that I was passed and I must go and tell my brother and sister the love of Jesus to encourage them to the house of worship. I saw a big roll of white linen put before me on the desk that I was to roll out and fold up again, and by the teaching of the spirit I was able to roll it out and fold it up. Then a white gentleman with white robes on—he was the inspector— stood before me and he said I was passed [in the first dream he was only 'convicted'; after he got the roll he was 'converted']. I found myself alone with the white gentleman and he giving right round the table wine and bread like as if we were taking communion at the house of worship. The angels appeared with plenty of white roses mixed with red and they showed me a white wrap for the head made of two yards of white cloth, and a small red cross for the coat. The Mammy wore a white cross, tassel, white dress, white wrap on her head. They sat around a spread table. sang and prayed. The angels showed a shout. The angels were standing working their arms and shouting while singing their beautiful chorus.

The angels teach songs from time to time. Sometimes no new songs for a long time, last year one new song. I saw a lighted room and in the lighted room I saw the angels and heard the singing,—

Sing and pray, sing and pray
The Lord is coming in the sky
With fire and brimstone.

Again a song,—

Children, you don' hear the bell?
What an awfu' sound!
Children, you don' hear the bell?
What an awfu' sound!
Jesus of Nazareth passing by,
What an awfu' sound!

[and so on, repeating the same words for "father," "mother," "brother"].

Mistress Dennis of Manchester, a young woman, is Mammy for all the districts. It is nineteen years since she received the spirit. She is the head of all the Mammy [in this section]. Each district has a Mammy and a captain, or sergeant, sometimes called "Pappy." Those who carry the staff are the captain and the sergeant,— some of the others according to the power given to them. The true Revivalists carry the staff. Some Salvation Army converts who do not belong to any army, because there is a regular army only in Spanish-town and Kingston, they join the Revivalists and carry their own staff. They have not the power of the Revivalists; they have not the power of revelation. When the staff was handed to me to give to others, it was told me if they don't be worthy of it, Christ will make the staff into a person and put into my place in heaven. There are many who get the power of healing. Most carry the Revivalist staff, but they may have a staff like a sword. They never carry it out, but leave it at home lying on a table with roses around it. I have not that power. I carry a different staff because I am the captain and because the Lord gives me power to see what is going to happen. I know when a person is going to die in the district. I don't tell the person himself, but the family. The person always dies when the word comes.

So much for the actual testimony of the people themselves. It was clear that the main business of the convert to Revivalism was that of "getting the Spirit"; but aside from vision, prophecy and healing, what was really the "work" of the white-turbaned Mammies to whom Margaret Williston and Alexander Edwards had so pointedly alluded? How did the doctrine of the group really differ from that of strictly evangelical churches?

Why all these night meetings and this emphasis upon the emotional reaction in hypnosis? Why the color antagonism? Inquiring into the expressed disapprobation for Revivalist doings of old Hannah French, the colored housekeeper, I got some clue to these questions.

She told me, and very nearly in these words, that in contradiction to her own church teaching the Revivalists believe God to be not alone, not omnipotent. There are evil spirits opposed to God. There are the spirits of the dead who are not at rest. Wicked people use these spirits to do harm to others. The Revivalists think by singing and dancing that they overcome these spirits, and this is the reason for their service. Many are terrified and fall down. They tear themselves for fear. This they call "getting the spirit." Thus I gathered from Hannah that the Revivalist group was a church militant battling not against men's sins but against the very demons who were the cause of death and sin in the world.

The belief in such spirit avengers is the basis of the hold which sorcerers have over the imagination of the negro in Jamaica.[2] Old Hannah, who had her share of superstition in the matter, said:

> Black people are full of wickedness. They want to revenge themselves on those that do them hurt. That is why they go to the Obi-men. Sometimes they will call up a dead parent to avenge them.
>
> They believe the spirit may come back at any time; especially after the burial it will come. The Adventists say for forty days, others for three days before it passes to another life to be with Christ.
>
> To call the dead back they say all manner of nonsense over the grave. They think the dead can hear. They entice the shadow from the grave with food and rum. To send it away they boil something together. They catch it in a bottle.
>
> Sometimes they bury the dead with a knife so that the dead can kill an enemy. A man at Newton, near Lacovia, was killed; so they did this

[2]Cf. A. J. Bell, *Obeah*, London, 1889; M. G. Lewis, *Journal of a West Indian Proprietor*, London, 1834, 134-138; 145-150; 329-330; 350-357; 390-391; J. S. Udal, *Obeah in the West Indies, Folk-lore*, XXVI, 1915, 253-295; *Obeah Worship In East and West Indies, Folk-lore*, IV, 1893, 207-213.

For equivalent African beliefs, cf. A. C. Hollis, *The Nandi*, Oxford, 1909, Introduction by Sir C. Eliot, xxi-xxiv; A. J. N. Tremearne, *Ban of the Bori*, London, 1914; Duff Macdonald, *Africana*, London, 1882, 204-214.

See Dayrell, *Folk-stories from Southern Nigeria*, London, 1910, 4, 139, for notes describing the *Juju* or medicine-man of the Egbo society.

In E. Jacotet, *Treasury of Basuto Lore*, Part I, 268-269, the *obe* is described as "a fabulous animal of very large size in the service of the witch."

The Portuguese negro term for the obeah-man is *Saibo*, possibly from the same source as West African *sasabonsum* (*abonsum*: malignancy, witchcraft). These are monstrous beings of human shape who eat human flesh and are found in silk-cotton trees or where the earth is red. See A. B. Ellis, *Tshi-speaking Peoples of the Gold Coast of West Africa*, London, 1887, 35.

to see who had harmed him. The dead spirit came back. They had to call him off or he would have killed the man.

One boy found that someone had meddled with his fish-pots. The Obi-man told him to call up his dead father, didn't warn him, and he went and handled the fish-pots. So when the dead father came, he hurt his own son. They had to get some one to call him off. The boy's name was Campbell at Red Hill.

Another old woman, Mercilla Hopkins at Brownstown, said of those who practise the art of sorcery in order to harm others:

Before they die they must confess everything. When Mary Gulla was dying, all her friends went to see her. She told them they must stand from the door and let her sins come in. And they must look beneath the bed and see some pins and needles; they must bring them within view. So now in every wicked act she had a part she must confess. A little boy fell off his horse in the race-course and she confessed that she did it. But she couldn't see the Lord's face and she said her soul was lost.

John Taylor confessed many wicked acts, said he killed many people. He was a hearty sound man [when he died].

I asked her how the sorcerer got into touch with the dead spirit. She said:

The *myal* is what they do to compel the dead. They boil rice and put it for the dead and sing some funny sing. They [the dead] must come up and eat the rice. Then they catch the shadow of the duppy—can't catch the soul but catch the shadow. They can't catch the body—that rots; nor the spirit, for that goes up to God; but they can catch the shadow. They put it into a bottle and cork it up.

From these accounts it would appear that what the Revivalists are doing is to take over the demon-driving business of the *Obi*-men. Having got hold of a more powerful *Obi* than was known to the old enchanters, they are disposed to work it for the exorcising of all demons of evil abroad in the world. And no one can hear the beat of the drum and the rhythm of the Revivalist song after it has slipped the leash of its English cadence and become a repeated cry out of the heart of the supplicant; can see the rocking swaying bodies of the worshippers in the dim light, dizzying the senses; and listen to the sharp hysteric barking of those who are possessed by the Spirit; without feeling that here we are touching near the bed-rock of that emotional heart of the race upon which our own religion has reared its superstructure of Christian theology.

How close the Revivalist methods are to African beliefs and practices may be seen by comparing them with the reports of the best first-hand observers in the field. A. B. Ellis, the chief authority on the negroes of the West Coast, says that among the Tshi-speaking negroes of the Gold Coast, from which group the Jamaica negroes are largely drawn, dancing was a special branch in the education of priests and priestesses and was engaged in for the purpose of curing sickness. It was always accompanied by the drum. During the performance, the priest

was supposed to be entered by a god; if he fell down, it was imagined that a second god had entered him and was engaged in a struggle with the first.[3] According to Weeks, a missionary in the West Congo, men and women parade at night to remove sickness.[4] Tremearne describes the highly specialized ghost dances practised among the Hausa in order to persuade the *Bori* or spirits to enter the dancers. Hysteria is induced as a sign of spirit possession and the person thus possessed is able to foretell the future.[5] Moreover, a similar color symbolism is observed. Ellis says that white is the proper color for the clothing of priests and priestesses at festivals of this character. According to Tremearne, ghosts of the dead are clothed in white. Among the *Bori*, each spirit is supposed to be attracted by a special color, just as among the Morocco Jinns color is a distinguishing character of the special spirit who enters a man.[6] The hysteria or trance is induced for purposes of healing. Klunziger mentions "a form of religious intoxication to which the Soudanese are particularly susceptible, in which dancers fell into ecstasy from the repetition of the name of Allah." This happens when they hold a dance at the home of a sick woman "under the direction of the *shecha* of the *sar* or Jinn who produces sichness." [7] A writer in the *Journal of American Folk-lore* for 1913 finds among the mountaineers of East Tennessee "a custom of those already converted to 'die' for their friends." This consists in "falling into a trance and remaining in such a condition for some hours (in one instance eight); the time of course varying with the hard-heartedness of the one for whom the exertion is made."[8] Compare this Christian sacrifice with Monk Lewis' account, in his *Journal* for 1816, of a *myal* performance in Jamaica, in which the sorcerer first pretends to kill and then restore to life again a man who has offered himself for the purpose.[9] "To show his power," says Lewis of the sorcerer's motive in presenting this spectacle of death and resuscitation; but there must be in the negro's mind a belief that the spirit of the man in trance goes out of him to war with the demons who are tormenting the sufferer. Margaret Williston and the other Mammies "work" in the same way. In symbolic fashion they "die" for those for whom they are struggling, whom they believe to be literally possessed of a devil.

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[3]Ellis, *op. cit.*, 121-126.

[4]*Folk-lore*, XX, 1909, 466.

[5]Tremearne, *op. cit.*, 20, 145-151, 424-426, etc.

[6]*Ibid.*, 142, 150; Ellis, *op. cit.*, 124, 222.

[7]K. B. Klunziger, *Upper Egypt*, New York, 1878, 394-396.

[8]*JAFL*, XXVI, 1913, 145-146.

[9]M. G. Lewis, *op. cit.*, 354-356.

II. The Isaiahs

The Baptist clergyman's wife at Brownstown, in the parish of St. Ann on the north coast of the island, directed me to a religious group living some eight miles out of town in a place called Tricey and designating themselves "Isaiahs" because of their leader's claim to direct inspiration from that prophet. The lady told me that, when her husband first came to Brownstown, he found the Isaiahs worshipping in the open, a rock as their altar, upon which they sacrificed a goat and performed other strange rites. This happened in the old days of Edward Alexander, the first Isaiah, who was converted "just after slave-time" (in 1838) and predicted the coming of the revival in 1860. His son James who succeeded him, the present Isaiah, was already an old man at the time of my visit. His small congregation met on Sunday morning in a neat church built after the English pattern. Except that some members of the choir carried wands of the Revivalist pattern, although with a simpler twist to the knot at the top, and rocked their bodies as they sang, the service differed in no way from that of any independent negro religious meeting. But within the churchyard stood an old tree and the historic stone slabs about which the earlier Isaiahs had worshipped. And in substance the story which old James Alexander told conformed closely to the Revivalist pattern. First came the conversion through vision, as if under direct control of the Spirit. Then the continued proof of inspiration in the power of fore-knowledge and of healing.[10] It was clear that the leader must, as Edwards said, first get the dream and then give it to others. His leadership depended upon his acting as a vehicle through which the will and the knowledge of God were imparted to the people. I give the story in his own words.

> It is exactly forty-one years since I was converted. I was called by the spirit of God. As I was called, I was down on my back in one place in the room eleven months. I couldn't come out at all, only lying in one place, the spirit taken away from me and I learning the mystery of God.
>
> Then one day I got Psalms 116. Then I rose and came outside the door and saw my two little boys there, and I stayed from eight in the morning until four o'clock. After that I went back to my bed and lay down. And after I lay down, in the night, I was taken away in the spirit to Brownstown. I didn't walk 'pon the earth; I flew in the air to Brownstown, I and the Master in the spirit. And when I got to Brownstown, I saw the Master. And he had a little book and he spread it upon the earth and I couldn't see. And he read out of it, "Parson Clark, your time is up on earth." And I saw a beautiful vessel waiting for him. And then he read and he read out of that book. I couldn't understand and he said, "No, indeed, for it is Latin and parable; but I will speak plain that you may understand." And when he spoke, I saw the foundation of the house move off. And he said to me, "It is finished with Parson Clark." And the foundation of Brownstown groaned.

[10] Cf. J. M. Fletcher, *The Miracle Man of New Orleans*, this Journal, XXXIII, 1922, 113-120.

And when we rose, he brought me to the church in Tricey and we didn't come 'pon the earth. And we saw a number of children like sheep. And he said, "Now take charge of these sheep and mind them for me." And a number of these children lay down in one place and some were away off. And I said, "Some of these sheep are very trespass." And he said, "You desire to bind these trespass sheep with cords, and the cords are the word of God." And I said, "Sir, when it become dark, how shall I see to mind those sheep from the Lion and the Wolf—from the Devil and Satan?" And he burst out with a great light which passed to the four corners of the world. Then the Master departed and I saw him no more.

So I am bound by the spirit of God with these people. I take no money for these forty-one year, take no money from nobody. I go from one place to another—quite down to Macedonia; but no night work, all in the day, on Sunday. The band goes along with me and must be back before night.

The great earthquake that is past [in 1907], the message was given me to warn the people. And I warned them three weeks. And the Bible opened at Numbers 16—"To-morrow ye shall know who is the Lord."

And an old lady [an elder in the church], the last that is gone away, was in the church with me and the spirit said to her [through him], "Mother Hall, the angel comes for you." And she rose up and took the message. And the next Sunday she was not able to come out and she lived only two weeks. Old? Not too old.

My dear wife here, she was sick; vomited blood. I went to the field to work and fretted lest she die behind my back. One night I took a journey into a world I didn't know and I saw a great house before me. When I entered the door, I saw door, door, door, door, but I heard no voice of anyone. And I saw a lady coming with her hand up *this* way. And as she came to me she said, "Take this," and I received it. And she said, "Isaiah, take this and boil it, and go to the doctor's shop and buy a six-pence worth of paregoric. And after you boil it, measure how many wine-glasses the bottle contains. When you boil it, don't put it on your table, lay it under your bed-head and let Mrs. Alexander take a wine-glass in the morning and one in the night. And I say, this shall keep down the stomach from bleeding." And I did so. And just as the bottle ketch in the middle, she said, "Father, I don' want any more." And the blood stopped until this day. No more blood.

III. The Bedwardites

Upon reaching Kingston in December 1920, I found the city in a state of half-amused, half-expectant excitement because a certain negro prophet named Alexander Bedward had predicted his own ascent into heaven, the destruction of the whites, and the reign of Bedwardism upon earth. These climactic events were to take place on the last day of December of that very year. The faithful were exhorted to gather into the village of August Town, at the foot of the Port Royal mountains near the river Mona, a few miles out of Kingston, where stood Bedward's church and where for thirty-two years had centered those religious activities of the prophet which had made the "healing stream" of the Mona river and the baptisms held in its life-giving waters celebrated all over the island and had gathered a large number of converts to his teaching. In response to the prophet's call, those living in other parts of the island were now rapidly selling out whatever of their property was not trans-

ferable and flocking to the central camp at August Town to be ready for the predicted destruction, which was to fall upon none, white or black, who stood within the sacred enclosure destined to become the center of the new Jerusalem.

In the city, the wildest rumors were afloat in regard to the prophet's real intention. It was confidently asserted that the man was a swindler and meant to steal away out of the island on the fateful day with the wealth which he had wrung from the credulity of the people. If a fire-balloon went up at night, the jest went round that this was Bedward ascending into Heaven. Even the intelligent whites believed that something out of the ordinary was about to happen. Among educated colored people I heard one reminding another wistfully of another Christmas day and of another prophet who was to the wise foolishness but whom the common folk had heard gladly. The negro entertainer at the Flamstead estate where I spent Christmas day, enumerating the marked events in his life said, "I remember the St. Thomas rebellion in [1865] and the Revival of 1860. It was taken up by the whole world. Now to-day there is Bedward!" The Kingston *Gleaner* wrote under large headlines:

> Yesterday there was great excitement at the Railway. Every train brought in a large number of people, men, women and children all bound for August Town. But they come from Colon as well, on a steamer, travelling across seas to see their 'Lord of August Town' ascend. In King Street all tram-cars travelling to Hope Gardens were besieged by men, women and children, some infants in arms, others hardly able to help themselves, but they were bundling in with with their clothes, baskets and fowls. All have sold out to come up and see the Lord and Master do the disappearing trick. There are still more to come.[11]

Many feared that the occasion would give pretext for a negro uprising against the whites.[12] One and all warned against visiting the prophet at August Town at this time. Nobody could tell what the negroes might do in their excited state.

When, on the morning of December 26, I walked over alone from the end of the Hope Garden tram-line by a foot-path to August Town, I found the churchyard crowded with campers, and the bed of the river—a mere trickling stream because of the drought—alive with folk. Women were washing clothes, rubbing them clean against a rock. Old men lay stretched on the bank asleep. Children went to and fro with water-cans for the camp, and donkeys ambled along panniered for the same purpose to the shout of "Donkey! donkey!" to clear the way. The guard at the gate who at first refused entrance to anyone un-

[11] *The Gleaner* (Kingston), December 29, 1920.

[12] The St. James rebellion of 1816 was supposed to have been incited by the inflammatory preaching of the Methodists. Cf. M. G. Lewis, *Journal of a West India Proprietor*, London, 1834, 226.

ready to "bow before the Lord and Master" was easily pacified, and an escort was found to lead me to "the Lord." The prophet was at the moment exhorting the people from the verandah of his house. So great was the press about him that it was ten or fifteen minutes before I could gain an interview. Meanwhile the folk scarcely observed my presence. On their faces was one fixed and consuming desire—to see their Lord, to hear him speak. "Lay hold on the Lord's word," was their assurance.

Bedward had at this time changed his title from that which he had so long borne as the "Shepherd," and announced himself as the incarnation of Christ himself. He was to be addressed as "Lord." When I finally sat beside him inside his house, he pressed this point with a touch of asperity. "I myself am Jesus Christ; I was crucified," he answered sharply when I hazarded an objection based on the teaching of Christ.

The man had a grievance. Several times he had petitioned that his own pastors be licensed to perform the marriage ceremony. In this way the marriage fees from members of his congregation would come into the hands of his own church instead of into those of outsiders. This privilege had at length been definitely denied him and he had accordingly decided to bring the world to an end. He showed me his black hands and told me how, in the new Heaven and the new Earth, they were to be as white as my own. He was a large fine-looking negro with the rapt brightness on his face of a visionary, clouded by the peevishness of tyranny. I looked into his face in vain for any hint of craft.

He told me of his past. "As a young man I committed every sin but murder; of murder I was not guilty;" and the story of his conversion followed as it is told by his biographer. He spoke with great simplicity of the symbolism of his church rites—the candle and the three fast-days a week—one as a symbol of the Trinity, the other designed for the purification of the soul in the spiritual life as of clothes in the secular. He said, "The tallow is like God; the wick within the tallow is like Christ in the bosom of the Father; the flame is the Holy Ghost who came from the Father and the Son." And of the fasts, "A day for washing, a day for drying and starching, a day for ironing; so the heart is made clean by the fasts."

All was gently, even reasonably spoken except for that moment of pettishness about his identity with the Lord. He spoke like a man who says a thing over until the repetition itself fastens conviction upon him. Far from being, as the scoffers say, a scheming pretender fattening upon the credulity of his followers, he was himself the most duped among them. He was

a dreamer, now so far gone in senility—he must have been about sixty-two years of age—as to have dreamed the impossible.[13]

That this view of Bedward is correct was later proved by the events of the fateful 31st. He must have done absurd things those last days. It was said that he had a woman tied to a tree all day learning to fly, and reproached her that her faith was after all insufficient. On the morning of the day predicted for the ascent, he took a chair and stationed himself in the yard in full view of his followers, who grouped about him dressed in their fast-day clothes of white. As the hours passed and nothing happened, he told them that the event had been postponed. From time to time he set another date. Still many remained faithful. In May, he determined to make a public demonstration of his claims. On the march into Kingston, the government seized the opportunity to arrest the leaders for vagrancy, consign Bedward to the Insane Asylum, and disperse the prophet's following.[14]

It is likely that this event will close the history of Bedwardism in Jamaica. The story of its founding, organization and doctrine may be read in a small pamphlet published in Kingston in 1917 and written by an intelligent negro missionary named Brooks, who had become a convert to Bedwardism.[15] According to this account, the founder of the religious group was a negro named Woods, but commonly called Shakespeare, who came from America some time before 1879, made his home in a stone-hole and gained some credit for his prophecies. He first predicted a flood, which presently followed. In December 1888 he visited August Town and threatened to sink the place unless the people should repent. They gathered to a meeting. Woods promised them the Spirit, and a strong wind coming up at the moment, his success was complete. He organized the church at August Town by appointing as elders twelve men and twelve women, one of whom he predicted would succeed him. Alexander Bedward was the destined successor.

Born in 1859, Bedward spent his youth on the Mona estate where he was known as "a good workman but a wild liver." After two years in Colon, where he got relief from a disease that had been troubling him for thirteen years, he returned to Jamaica only to be seized once more with the disease. At Colon, once more, he had a vision ordering his return on a mission to August Town. Hesitating to obey, as Brooks writes:

[13]The Editor reminds me of Lord Frederic Hamilton's reference to Bedward in his *Here, There and Everywhere* (4th ed., 156) in connection with the earthquake of 1907, which suggests a relation between earthquakes and religious conversion.

[14]*The Gleaner* (Kingston), Thursday, May 5, 1921.

[15]A. A. Brooks, *History of Bedwardism or The Jamaica Native Baptist Free Church*, Kingston, Jamaica, 1917.

During the evening, he and several others were sitting outside in the open air when he alone saw a man in white apparel approach him, and said "Did not I send you to those men for money to return to Jamaica? Why have you not obeyed?" Said Bedward, "Yes, but I know they are not going to help me, therefore I did not ask them." Then the man produced a whip with which he inflicted so severe a chastisement on Bedward as made him agonize. Those standing by observed him with extreme astonishment, hearing him speak to some, to them, invisible person and seeing his agony.

Bedward began his public ministry in August Town by performing baptism at the Mona river and giving the water as "medicine." Brooks says:

The Lord told him saying "Once I made water wine. Behold, now I make water medicine. And you have I ordained my Dispenser, Watchman, Shepherd and Trumpeter."

Of the two hundred persons who took the water that day, only seven had the faith to take it as "medicine," and these seven were instantly cured.

In 1894, the corner-stone was laid of the "Jamaica Baptist Free Church," the attractive stone building 61 by 91 feet in size which now stands within the enclosure of the Bedwardites in August Town. When Woods died in March, 1901, he left Bedward ordained as "Bishop" of this church, with the title of "Shepherd." There were appointed, besides, two pastors, twenty-four elders and seventy-two evangelists. "Station guards" and "Mothers" were also named, and in various parts of the island "Camps" were established for affiliated congregations.

The church ritual laid emphasis upon fasting and upon special purification ceremonies to precede baptism. It appointed every Monday, Wednesday and Friday as a fast-day from midnight until one in the afternoon. At five in the morning the fasters assembled in the church for prayer. At twelve noon, those dressed in white gathered about a table covered with a white cloth on which were set white cups and saucers; others found seats elsewhere. Bread and water were distributed. After a praise service, at the minister's words "Are all in peace?" a grace was said, and all lifted their cups and broke their fast.

Immediately preceding the ceremony of baptism, which occurred every three months, a three days' fast was imposed participation in which was voluntary. It began on Tuesday at five and lasted until Friday at three. About noon on Friday, the participants, dressed in white, gathered in the yards with their cups. Here they marched and sang while the cups were filled from jars of water set ready for the purpose; then, at a given signal, all tossed the water into the air and marched into the church, where the usual ceremony of breaking fast was observed. Just before baptism, a "vow ceremony" was observed, when the "earnest inquirers" ranged themselves at the altar and

received a lighted candle. Then all repaired to the river where the immersion took place, the candidates being escorted by a singing procession dressed in white and carrying lighted candles in their hands.

Bedwardism taught obedience as its foundation principle. Of the "three heavens" prepared for the believer, the first was to be gained through "blood," the second through "love," the third only through "obedience to the teachings of Bedward." Much in these teachings emphasized the Holy Ghost as the third revelation of the Trinity. "Judaism, Christianity, and Bedwardism are in the order of the Father, Son and Holy Ghost for the retrieve of fallen man," says Brooks in his pamphlet.

Much about Bedwardism as laid down in this source, such as the organization of the church on the Apostolic plan into Shepherd, ministers, elders, and evangelists; the emphasis upon the outpouring of the Holy Ghost; the constant fasting and communion, suggests the influence of the Apostolic Church, branches of which have, as we know, survived in America.[16] Woods may have got his ideas from this communion. But there is also much to be found in common with other religious cults which have gained power over the negro mind in Jamaica, much that harks back to an earlier religious symbolism. It was in vision that Bedward received his call, and many of his converts testify to the "vision of the white cup" before conversion. The power of prophecy, the power of healing, are his guarantees to the spiritual calling, and it is by the use of medicinal water that the healing is effected. The white dress and the candle are symbols of harmony with the spirit. The ceremony of tossing water into the air precedes that of Christian baptism. In the church enclosure stands a cottonwood tree whose roots clasp a rock. "A fountain will break out from this stone" says a prophecy: and although this, say later interpreters, must be taken in a spiritual sense, it is worth observing that the rock and the tree and the spring of water enter into the religious symbolism of the church and are preserved side by side with the house of worship built after the Christian pattern.[17]

[16]United States Census. Religious Bodies: 1906, II, 179-181.

[17]Murray, in her *Witch-cult in Western Europe*, Oxford, 1921, refers to an interdict published in England in the 8th century against "vows paid, loosed, or confirmed at wells, stones, or trees" (22); to dances about a stone in witch rites (131); to candles employed in the ritual (144); and to rain-making (172). Cf. A. J. N. Tremearne, *Ban of the Bori*, London, 1914, 234; A. B. Ellis, *Tshi-speaking Peoples of the Gold Coast of West Africa*, London, 1887, 82.

A STUDY OF THE MÜLLER-LYER ILLUSION, WITH SPECIAL REFERENCE TO PARADOXICAL MOVEMENT AND THE EFFECT OF ATTITUDE[1]

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The history of the Müller-Lyer illusion dates from 1889 when Müller-Lyer published his article on "Optische Urteilstäuschungen."[2] In the year following, Brentano called the attention of psychologists to the problem and for several years thereafter the illusion was much discussed. The first serious experimenting and the most important research made on this particular topic in the last century was that of Heymans in 1895.[3] The most important work done in the present century is that of Benussi, 1904-1918.[4]

The problem of the Müller-Lyer illusion has never been fully defined. Either it has not been clearly thought out by the investigators and theorists who have dealt with it, or at best it has been variously stated, according as it issues from different systematic schools or represents different theoretical points of departure. Indeed, to state the problem is usually to hint at its possible solution. Everyone would, however, agree that the problem is to discover why two lines of equal lengths are reported as of different lengths when accompanied by certain *Nebenreize*. Not everyone would agree, however, that this statement is precise, or that it does not include more than is relevant to the problem. The psychologist, unless he is a behaviorist, asks: "What are the psychological processes which underlie the verbal report, and how are they dependent upon the *Nebenreize*?" Or perhaps: "Does the bare visual extent of the Müller-Lyer figure vary with the *Nebenreiz*, or is it merely the judgment upon the psychological extent that depends upon the *Nebenreiz* and thus renders the experience 'illusory' ?" Even this latter statement, however, implies a systematic belief and does not entirely overcome the difficulty, for certain psychologists assert that judgment and bare experience can not, at least in practice, be separated from each other, and deny that simple sensations are directly correlated with the physical stimulus. It would seem, then, that any attempt to state the problem with precision launches us at once into the larger issue of what perception is, and that a study of the Müller-Lyer illusion can be undertaken only from a particular point of view. Indeed, it is not so much the fact that the theories about the illusion are not all directed towards the same end or that they have grown out of different pre-

[1]From the Psychological Laboratory of Clark University.

[2]F. L. Müller-Lyer, Du Bois-Reymond's *Archiv*, 1889, Suppl. Bd., 263ff.

[3]G. Heymans, *Z. f. Psychol.*, 1895, 9, 221-255.

[4]V. Benussi, Meinong's *Untersuchungen zur Gegenstandstheorie*, 1904, 303-448; *Z. f Psychol.*, 1916, 42, Abt. 1, 22-55; *Arch. f. d. ges. Psychol.*, 1912, 24, 31-62; *ibid.*, 1914, 32, 50-57; *ibid.*, 396-419; *Z. f. Psychol.*, 1914, 69, 256-292; *Arch. f. d. ges. Psychol.*, 1918, 37, 233-282.

conceptions as to the fundamental nature of the problem, as it is that the basis of many of these theories has not been described nor the meaning of the terms used adequately defined. Nor in systematic matters is agreement so much to be desired as clarity in disagreement.

In general there may be said to be two principal types of theories: (1) those theories which assign as the cause of the illusion some further phenomenon which is separate and distinct from the actual phenomenon of the illusion but in the absence of which the illusion should not occur, and (2) those theories which have as their basis certain mechanisms under which the phenomena of the Müller-Lyer illusion can be subsumed, and which are therefore simply names for a group or groups of well established facts. Under the latter head can be placed that type of explanation which makes its appeal to laws rather than to mechanisms—laws which state merely the conditions of occurrence of the illusion and show, when possible, how they are identical with the laws of optical illusions in general.[5]

Benussi holds to the second type of theory and rejects the hypotheses thus far put forth.[6] He contents himself with a statement of the laws of what he calls 'the inadequate perception of form.' Only by reference to a careful summary of fact, so he feels, shall we be able to arrive at a satisfactory explanation of the reason why illusions occur. One may add that the problem of modern science is not so much the 'cause' of a phenomenon as to be able to state as accurately as possible the conditions for its occurrence. "The word 'theory' no longer means an hypothesis deliberately added to observed facts but an actually observed law or set of laws from which deductions may be drawn." [7] According to this view, then, the laws which Benussi has formulated are quite as deserving of the term 'theory' as the more picturesque explanations of earlier investigators.

The difference between the more recent theoretical work done with the Müller-Lyer illusion and the earlier work seems to represent a change which has taken place in fields of scientific inquiry in general. The feeling seems now to be that generalizations must wait upon the discovery of facts, and that while speculation may not be harmful it must be judged only for what it is worth.

It was not expected in the present investigation to make discoveries which would lead to a new theory of the Müller-Lyer illusion but rather to contribute some additional facts to the body of knowledge on the subject. Especially was it felt that new facts might come to light if the problem of the Müller-Lyer figure were undertaken in a laboratory which had at its disposal subjects trained in the introspective technique.

I. Paradoxical Movement

The first part of the experiment was undertaken in order to study introspectively paradoxical movement between two members of the Müller-Lyer figure shown successively in a tachisto-

[5] For a general discussion of theories see: V. Benussi, Meinong's *Untersuchungen zur Gegenstandstheorie*, 1904, 414-448; V. Henri, *L'année psychol.*, 1896, 3, 495-510; 1897, 4, 538-542; H. Piéron, *Rev. phil.*, 1911, 71, 245-284; E. B. Titchener, *Qual. Instr. Man.*, 1900, 321-328. A full discussion of theories from the point of view of the paragraph above is contained in a bound manuscript, by the present author and with the present title, that is deposited in the Clark University Library and may be borrowed under the usual courtesies of library exchange.

[6] *Arch. f. d. ges. Psychol.*, 32, 403ff.

[7] E. B. Holt, *The Concept of Consciousness*, 1914, 129.

scope. Such a study had already been conducted by Koffka and Kenkel,[8] but it was hoped to get some further light on the exact structure of such a phenomenon, and especially to study the content-aspect of the visual experience.

The *O*s who took part in the experiment were *B*, Dr. E. G. Boring, *D*, Dr. L. D. Boring, *Y*, Dr. M. Yokoyama, and *H*, Dr. H. M. Halverson, all highly trained in introspection.

The exposure apparatus used was a modification of the Dodge tachistoscope.[9] It was built so that three separate fields were successively exposed, the first containing the fixation point. Each experiment began with the exposure of this field, during which the ready signal was given. A pendulum controlling shutters was then released. After an interval, which did not exceed 20σ, the field containing the first figure appeared. This field was exposed for about 7σ and then after an interval of about 30σ the second field appeared for the same length of time as the first. Each stimulus field was optically 42.5 cm. away from the eye of the *O* and the maximum length of any of the stimulus figures was 2.8 cm., so that the greatest visual angle subtended by any figure was $2°20'$. The total field was 3.6 cm. square and subtended a visual angle of $3°3'$.

A preliminary series was tried with the figure in which the extents to be judged were horizontal black lines, but this attempt proved unsuccessful chiefly because it was necessary to make the lines so wide in proportion to the size of the figure that the exact points of termination of the extents were indefinite. The main series was done with figures which consisted of black dots from 10 to 18 mm. apart and about 2 mm. in diameter. Certain of these figures had arrow-heads attached, others had feather-heads, and the rest had nothing at all. The *Nebenreize* were all 5 mm. long and ½ mm. wide, and made an angle of 30° with the horizontal. Seven pairs of stimuli were used as shown in Table I. In the presentation of the stimuli the *Konstanzmethode* was used.

The instruction was as follows (general): "At the signal 'ready' you are to fixate the black dot in the centre of the visual field. Immediately after the signal 'now' you will be presented visually with two successive pairs of dots, black on a light background. According to your impression, you are requested to make judgments of the comparative extent between those dots, reporting 'greater,' 'equal,' or 'less,' according as the second extent is greater, equal or less with respect to the first. In case of inability to make a judgment, report failure and then dictate your reason to the experimenter." Additional instructions: "Be sure to speak out during the experimenting whenever you think there has been anything unusual about your experience, or whenever any change in your attitude or mode of judgment is noted."

After Series II had been completed *O* was asked to make comment on every judgment and not merely on those he considered unusual. For this reason it was not found possible in Series III-VII inclusive to obtain more than ten judgments on each stimulus. Accordingly the points of subjective equality had to be approximated graphically rather than accurately determined by the usual mathematical procedures.

[8] F. Kenkel, Untersuchungen über den Zusammenhang zwischen Erscheinungsgrösse und Erscheinungsbewegung bei einigen sogenannten optischen Täuschungen, Koffka's *Beiträge zur Psychologie der Gestalt- und Bewegungserlebnisse*, 1919, 6-98.

[9] R. Dodge, *Psychol. Bull.*, 1907, 4, 10-13; F. L. Dimmick, this JOURNAL, 1920, 31, 318f.

Table II shows the relative frequencies and Table III the upper and lower limens, as well as the points of subjective equality (S.E.), obtained

TABLE I
Arrangement of Series

Series	Standard Stimulus mm.	Comparison Stimulus mm.	Order of presentation
I	16	14,15,16,17,18	Dots followed by dots.
II	16	14,15,*16,17,18*	Dots " " arrow-heads.
III	16	*14,15,16*,17,18	Dots " " feather-heads.
IV	16	*14,15,16*,17,18	Arrow-heads " " dots.
V	16	14,15,*16,17,18*	Feather-heads " " dots.
VI	18	*12,13,14,15,16,17,18*	Arrow-heads " " feather-heads.
VII	14	*14,15,16,17,18*	Feather-heads " " arrow-heads.

for each of the *Os* in each of the seven series. The values for Series II in the case of *D* were not obtained. S_1 for *D* in Series III was omitted because the stimuli used did not contain the upper limen; the same was true with S_2 in series V. In Series I there were 50 judgments on each stimulus for each *O*, and in Series II also, except for *B* who made 100 judgments on each stimulus. In Series VI and VII with *H* and in Series VI with *B* only 5 judgments on each stimulus were given. The comparison stimuli in Series VI for *B* and *D* were 12, 13, 14, 15, 16, instead of the normal 14, 15, 16, 17, 18 mm.

Table IV shows, first, the amount of divergence of the points of subjective equality from the points of objective equality in percentage of the latter, and secondly, the amount of divergence of the points of subjective equality of the Series II-VI inclusive in percentage of the points of subjective equality of Series I. These latter figures are the true percentages of the illusion in all the different cases, because they take into consideration the normal error made by every individual in the judging of extents under these conditions.

Charts I and II show graphically the data of the second part of Table IV. In all these presentations the illusion is said to be 'negative' when the effect of the *Nebenreize* is to make the second extent (comparison stimulus) relatively greater than the first (standard stimulus). The illusion in this case is negative because under such conditions subjective equality is less than objective equality. Conversely the illusion is 'positive' when the *Nebenreize* tend to make the comparison stimulus relatively less than the standard. The averages of Table IV and Chart II show the general relationship. (In the case of Series II, where a value was missing, the average was corrected in accordance with the average individual differences between *Os* in order to make it comparable.) Chart II shows clearly that the positive illusion is always less than the negative. Both the positive and the negative illusion follow the same law: namely, the effect of the feather-heads alone (III and V) is least, the effect of the arrow-heads (II and IV) is slightly greater, and the total illu-

TABLE II

Relative frequencies of judgments in each series for each observer, based on 10 judgments on each stimulus unless otherwise indicated. G = "greater"; L = "less." The number in parenthesis before the relative frequencies for *B* and *D*, series VI, indicates the change in comparison stimuli which had to be made in this series.

Series	I[1]		II[2]		III		IV		V		VI[3]		VII[4]	
Comparison Stimuli mm.	G	L	G	L	G	L	G	L	G	L	G	L	G	L
Y														
18	84	0	92	0	90	10	90	0	70	0	90	0	90	0
17	74	0	30	14	60	0	90	0	50	0	40	10	80	0
16	26	12	10	38	40	20	60	10	10	40	30	40	30	20
15	2	34	2	72	20	30	20	10	20	70	0	90	20	50
14	2	86	0	100	10	90	0	70	0	90	0	90	0	100
B														
18	98	0	98	1	100	0	100	0	90	0	(16) 60	0	100	0
17	90	0	66	1	90	10	100	0	60	0	(15) 20	0	70	0
16	20	28	31	18	50	0	60	0	10	30	(14) 20	0	20	10
15	6	50	3	77	0	30	30	10	0	90	(13) 0	80	0	70
14	0	92	0	94	0	60	10	50	0	100	(12) 0	100	0	90
H														
18	88	6	64	6	100	0	100	0	80	0	100	0	20	40
17	86	2	14	40	90	0	100	0	20	0	100	0	0	60
16	6	70	2	58	70	10	60	0	0	20	80	0	0	100
15	2	86	0	90	10	0	20	10	0	70	40	0	0	100
14	0	96	0	100	10	60	10	60	0	100	20	40	0	100
D														
18	80	0			100	0	100	0	40	0	(16) 100	0	90	0
17	58	0			100	0	100	0	10	0	(15) 80	0	50	0
16	14	20			90	0	90	0	10	70	(14) 40	0	30	20
15	8	20			70	0	40	0	0	90	(13) 0	100	10	70
14	0	74			40	40	10	60	0	100	(12) 20	80	0	100

[1] 50 judgments on each stimulus in this series with both *Os*.
[2] 50 judgments on each stimulus in this series with *Y* and *H*; 100 judgments with *B*.
[3] 5 judgments on each stimulus in this series with *H* and *D*; standard 18mm. for all *Os*.
[4] 5 judgments on each stimulus in this series with *H*; standard 14mm. for all *Os*.

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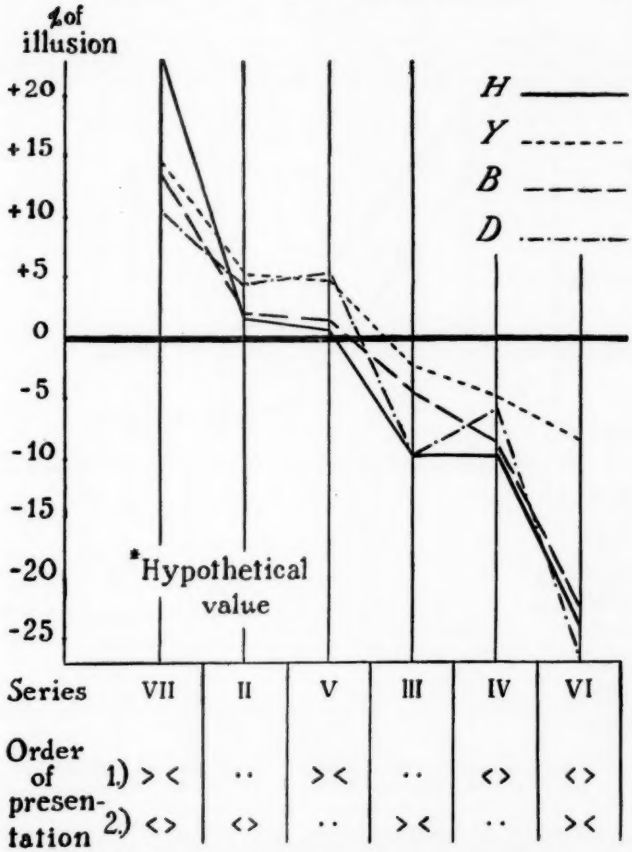


CHART I: Amount of the illusion, positive and negative, in percentage of extent of subjective equality without the *Nebenreize* (*i.e.*, corrected for time-error). Data from the second part of Table IV.

sion between arrows and feathers (VI and VII) is greatest of all, in fact greater than the sum of the corresponding two parts. Heymans[10] and others have shown that the arrows are less effective than the feathers and our results are in this respect consistent with others. The fact that the effect is more than doubled when feathers and arrows are opposed may be due to the fact

[10]G. Heymans, *op. cit.*, 235f.

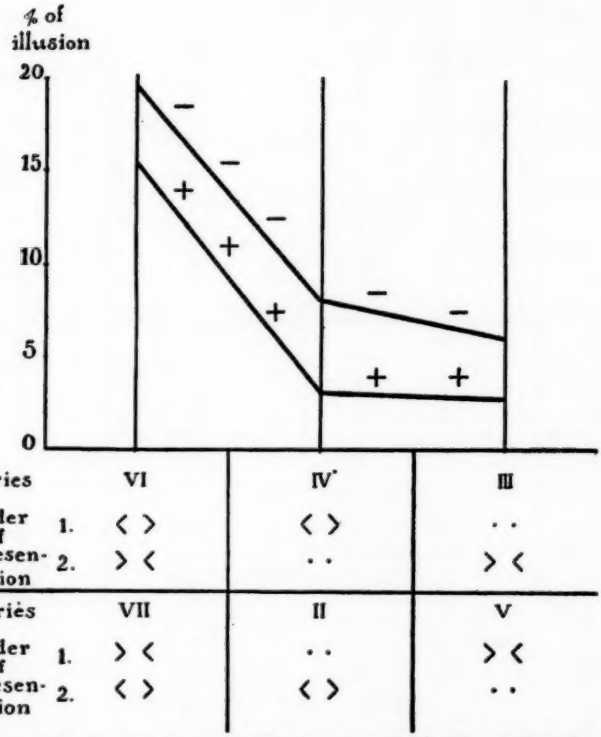


CHART II: Average amount of illusion, from Table IV and Chart I, arranged to show comparison of positive and negative illusion.

that in this more complex presentation (VI and VII) it was more difficult for the *O*s to maintain the attitude of judging process-extent, which they did in general by keeping the dots focal.

Summary of Introspections

The typical experience for all the *O*s was of a moving, changing figure, in which the first form changed into the second, a succession of separate figures.

Observer B. This *O*, while he worked with knowledge of the purpose of the experiment and often of his own results, showed such agreement with the other *O*s, not only as to the frequency with which he gave the various judgments but also as to the materials of his protocols, that his statements were taken at their face value.

TABLE III

Upper and lower limens and points of subjective equality based on values in Table II and determined graphically. S_1 = lower limen; S_2 = upper limen; S.E. = subjective equality; O.E. = objective equality (standard).

Os		*H*	*Y*	*B*	*D*	
Series						
I	S_1	16.2	15.0	15.2	14.6	
	S_2	16.8	16.4	16.2	16.9	
	S.E.	16.4	15.7	16.1	15.5	O.E. = 16
II	S_1	16.3	15.8	15.7		
	S_2	17.1	17.2	16.3		
	S.E.	16.7	16.6	15.8		O.E. = 16
III	S_1	14.3	14.8	14.3		
	S_2	15.6	16.0	16.0	14.4	
	S.E.	14.9	15.3	15.4	14.0	O.E. = 16
IV	S_1	14.2	14.4	14.2	14.3	
	S_2	15.8	15.7	15.6	15.2	
	S.E.	14.9	14.9	14.7	14.6	O.E. = 16
V	S_1	15.4	16.0	15.8	16.4	
	S_2	17.6	17.0	16.8		
	S.E.	16.5	16.4	16.3	16.9	O.E. = 16
VI	S_1	14.0	16.0	13.4	13.7	
	S_2	15.0	17.0	15.8	14.3	
	S.E.	14.3	16.2	13.8	13.8	O.E. = 18
VII	S_1	15.0	15.4	15.4	15.6	
	S_2	14.0	16.3	16.6	17.0	
	S.E.	14.0	15.8	15.9	15.9	O.E. = 14

The three criteria of judgment used by *B* were (1) relative displacement of the dots, (2) extent between them, and (3) movement outward or inward of the dots. Movement was either inferred or sensory; the sensory movement was that in which something was 'given' in visual quality, usually a gray, blurry connecting streak, presumably the process-basis of the phi-phenomenon.

I. In Series I most of the judgments were based on movement. The dots seemed actually to move in or out and the judgments were accordingly given as 'less' or as 'greater,' respectively. There were several cases, however, in which the judgment was contrary to the movement of the dots. Some indescribable criterion seemed to be present by which the *O* was assured that a particular movement was really illusory and should therefore be discounted. The dots were often reported as jerking, bobbing, or floating before they settled into place. It may be that this 'jerking' was the type of movement interpreted as illusory.

II. The judgments in Series II were made on the relative position or movement of the dots, but never on extent. Certain cases occurred in which the dots seemed to be left behind when the wings moved out or in. This made the judgment difficult for it meant that the dots in the original position could be compared either with the vertices of the wings or with the dots in the final position.

III. In Series III judgments of displacement and of extent were about equally involved in the critical judgments which showed the presence of

TABLE IV

Series	I	II	III	IV	V	VI	VII
Amount of the illusion in percentages. The sign indicates the direction of the illusion. When the point of subjective equality is above the point of objective equality the sign is plus, and conversely.							
Os							
H	+2.5	+4.4	−6.9	−6.9	+3.1	−20.5	−20.5
Y	−1.9	+3.8	−4.4	−6.9	+2.5	−10.0	+13.0
B	+0.6	−1.2	−3.8	−8.1	+1.9	−23.3	+13.6
D	−3.1		−12.5	−8.8	+2.5	−23.3	+13.6
The amount of the illusion as measured by the difference between the amount of divergence in series I and that in all the other series, between the point of subjective equality and the point of objective equality.							
H		+1.9	−9.4	−9.4	+0.6	−23.0	+23.0
Y		+5.7	−2.5	−5.0	+4.4	−8.1	+14.9
B		+1.8	−4.4	−8.7	+1.3	−23.9	+13.0
D			−9.4	−5.7	+5.6	−26.4	+10.5
Average		[+3.4]	−6.4	−7.2	+2.9	−20.3	+15.3

the illusion. Except in one case movement was observed only with those stimuli where the effect of the illusion was in the direction of the objectively correct judgment. There were, however, many cases where a 'flashing' or 'pseudo-movement' was reported, but was discounted by *O* as a criterion. There were also several other cases in which movement was reported but did not act as a criterion. These latter movements were not characterized as 'pseudo-movements' but may nevertheless have been such.

IV. Movement was reported in Series IV as the basis of several of the 'illusory' judgments. No contrary criteria were reported.

V. In this series, as in III, extent and displacement were the chief bases for the 'illusory' judgments; movement was given as a criterion only twice and then with the critical stimulus. Moreover, in one of these instances extent as a criterion operated in a direction contrary to the movement. In another instance extent as a secondary criterion opposed displacement as the primary, and in two cases just the reverse happened.

VI. All judgments in Series VI, whether illusory or not, were made on the basis of the criterion of extent.

VII. Extent was in Series VII the most frequent criterion, although it was relatively less frequent in those cases where the objective difference was reversed by the illusion. Movement was reported only where the comparison stimulus was of the same length or only 1 mm. longer than the standard. In these cases, however, movement was got distinctly and without reservation, and seemed to be used as a criterion nearly as frequently as extent and quite as frequently as displacement. All three criteria were readily effective and became equivocal only where the objective difference between the comparison and the standard began to be so great that the effect of the *Nebenreize* was weakened. *O* stated that he was most sure of those judgments which were opposite to movement. Objectively, such judgments were much more apt to accord with the facts than those which were made in correspondence with movement. He also reported 'pseudo-movement' which came both with the 'greater' and with the 'less'experiences, so that he distrusted its validity as a criterion for a judgment of 'less.' Here again this pseudo-movement may be related, at least, to the 'flashing' and 'darting' reported above.

Except in Series VII the criteria reported as having been at the basis of the illusory judgments were chiefly extent and displacement. In VI, however, we find movement reported as having occurred in the direction of the illusion without correction by any other criterion as in the other series. In series VI, which has the greatest illusion of all, extent was used entirely as a criterion, and in the other series it was used more than relative displacement for those judgments which showed the presence of the illusion. It would seem, then, that with extent focal the illusion is rather more likely to occur for this *O* than in any other case, except in VII where the use of the criterion of extent seems to have little influence on the judgment in either direction. According to this conclusion the strict following of the instructions rather seems to diminish the illusion than to aggravate it. The explanation probably is that bare extent is less easy to observe than either relative displacement or movement, and that possibly the *O* really was not able to narrow himself down to simple extent, but, when he used this as the basis of his judgment, was judging the impression more as a whole than when he used the other criteria. Indeed, the *O* himself said that it was more easy to report on movement, since movement was something 'given,' whereas one makes a judgment *about* extent. It might be noted here, however, that a certain amount of judging had to be done in deciding whether movement was true or 'pseudo.' On the other hand *B* remarked on one occasion that he was surer of judgments made contrary to movement as a criterion.

Observer H. *H* had the same three criteria as *B*, extent, movement, and relative position of the dots; but the first criterion he used only in the

first series. His experiences were usually complex and full of conflicts which he could not always resolve.

I. In this series *H* reported having judged on the basis of the successive positions of the dots rather than on the extent between them. He instructed himself to observe movement and reported many cases of simple movement, and also experiences in which no movement occurred. The gray of the phi-phenomenon came often, as well as a kind of jerking and dancing of the dots. There was usually a 'hang-over' of the first sensation which lasted into the second. Eye-kinaesthesis of contraction and of expansion took place. Both, especially the former, aided judgment considerably.

II. Judgments in this series, unlike those in the first series, were ordinarily made on the basis of extent between dots. Movement of the dots and the wings, however, was a factor in the judgments. Compromise often had to be made when the dots appeared to move out at the same time that the wings moved in, or when what seemed an illusory pulling in of the dots by the wings occurred. This pulling in, or 'tendency towards pulling in,' happened frequently, even sometimes with the largest comparison stimulus. In one case, however, *O* reported that, although there was movement inward, the comparison stimulus actually seemed larger in consequence, because the wings 'succeeded so miserably in bringing their dots closer together than the standard dots.' A good many different process-descriptions of the movement were made. There was also a shutting up of the wings like scissors, especially with the smaller comparison stimuli. 'Hang-overs' from the first presentation were still experienced.

III. In the first series movement was experienced, but not used as a criterion; in this series it was used as the basis for most judgments of 'greater.' These judgments were usually univocal, and occurred chiefly with the objectively greater stimuli; the judgments of 'less,' or 'equal,' which occurred most frequently with the objectively smaller stimuli, were based almost entirely on the position of the dots. There were some cases, also, with the smaller stimuli, where an outward movement of the wings was present, although the judgment was 'equal,' and one case of an equal judgment was given in spite of an outward movement of the dots themselves.

IV. Here, except in one case, the only stimuli judged on the basis of movement were the larger ones, where movement, along with position and occasionally extent, was one of the criteria. Position was the chief criterion with the 'greater' judgments, and practically the only one with the 'less' judgments. There were many reports of rather irrelevant 'flutterings,' 'flashings,' and 'shootings' of the wings. Many of the 'less' judgments were made *in spite of* these phenomena. Both these 'flutterings,' and the 'shutting up' of the wings reported in the second series are probably akin to *B*'s 'flashing' and 'darting.'

V. In this series relative position of the dots again becomes the chief criterion. However, it is likely that many of the judgments of 'less' and 'equal,' even when not so reported, were based at least partly on movement of the second pair of dots inward after they had appeared to be farther apart than the first pair. Several cases of this kind were indeed described. The only equivocal judgments, including those just mentioned, where a conflict occurred between relative position and movement, appeared where the influence of the illusion would naturally lead to objectively incorrect judgments.

VI. Some sort of movement was reported as present all through this series although ordinarily the 'shooting' and 'flashing' of the wings was at least partly abstracted from. Actual movement came in mostly with the larger stimuli, where the dots seemed to move outward even though objectively the largest of the comparison stimuli was smaller than the standard. No report was given after two exposures of the smallest comparison stimulus, possibly because of the lack of a positive criterion.

VII. Equivocal judgments were present in this series only where the influence of the illusion might be expected to lead to an objectively incorrect judgment. Most of the judgments were based on movement of the dots in or out. A 'flashing' of the wings, described here as like the closing up of the leaves of a book, and in Series II as like the shutting up of a pair of scissors, was always present and sometimes influenced the judgment. There were several compromise judgments where the dots moved out, but the closing up of the wings gave the impression of contraction, and in consequence the judgment 'equal' was usually given. There were also two cases where relative position as a criterion was set over against movement of the dots, and a compromise judgment of 'equal' given.

Throughout all of the series *H* reported conflict between his naive impression of the figures, in which movement seemed to play a large part, and a more analytical attitude in which attention was paid to the relative position of the dots and an attempt was made to abstract away from the movement.

The particular conflicts which occurred for *H*, where illusory judgments were given, were of a number of different kinds. There was the conflict involving 'pseudo-movement,' conflict involving movement inward of the dots after they had appeared farther out, and conflict between relative position and movement. Very likely several of these conflicts could be resolved into one, but *H* spoke of them in his introspections as if they were all quite different.

Observer Y. *Y*'s attitude, in striking contrast to that of *H*, was relatively simple and consistent, and admitted of very few conflicts and secondary criteria.

I. In series I the judgments were mostly based on relative position of the dots. *O* instructed himself to observe movement and reported this as well as illusory connecting lines between corresponding dots in successive exposures. These lines evidently took the place of palpable movement when they occurred.

II. *Y* reported in this series also that he made most of his judgments on relative position of the dots. Here there was no actual movement of the dots, but there were the same illusory connecting lines as in Series I, vague, grayish things running from each dot to the corresponding dot in the next exposure. Movement was reported, but only movement of the wings as they seemed to shoot out from the dots.

III. In this series *O* speaks for the first time of judgment on the basis of more extent between the dots. This criterion as well as that of relative position was used with the larger and with the smaller stimuli. Equivocal judgments, where extent and position did not both lead to the same judgment, or where the judgment was not made merely on the basis of one criterion, were found only with the critical stimuli where the illusion would naturally occur. In these cases the judgment made on the basis of relative position was always the objectively correct one, whereas that made on the basis of extent gave evidence of the influence of the illusion.

IV. All but two of the judgments in this series were made on the basis either of extent or of position. One of these judgments was made on what *O* called 'immediate impression of extent,' which seemed to be distinguished from the usual extent judged in the former series in that it was less analysed than those other impressions of extent; whereas the other of these judgments was made on a 'narrowing down of the brightness field' which immediately led to the judgment 'less.' Judgment on 'immediate impression' was 'greater' by this criterion and by relative position; that is, it was reported as being equivocal.

V. In this series extent, which the *O* now called 'immediate impression of extent' but which seemed to be merely the sort of thing he had always been judging, was the main criterion, although relative position came in often to reënforce the judgments made on the basis of extent. *O* always

seemed very well satisfied with the judgments which he made on the basis of extent alone; but, as a matter of fact, those made on both extent and position were more likely to be correct objectively.

VI. The main criterion here was also extent. Secondary criteria came in very seldom, although change both in the brightness field and in relative position of the dots was occasionally spoken of as helping out the judgment. The wings were reported throughout the series as rotating from one to the other position.

VII. Movement of the dots and change in their relative position were mentioned several times, but the greater number of the judgments were made solely on the basis of extent between dots, or 'immediate impression of extent' as it was still called. Rotation of the wings was experienced in this series as well as in the one proceeding.

For *Y* the main criterion throughout was evidently that of extent, which came to him as something absolute. The first figure was simply of 'that extent,' the second was at once seen as either greater or less than this first figure, or equal to it. Some of the introspections show, however, that the relative position of the dots had a good deal to do with this immediate impression of extent, although *O* thought that he would have got even fewer secondary criteria than he actually did, had he not been asked to make detailed reports on his experience. *Y*'s illusory judgments taken alone show only one thing which may be significant and which distinguished these judgments from the others: in Series V position as a criterion seemed to be more nearly 'correct' than extent.

Observer D. I. *D* took a passive attitude and judged mostly on the basis of movement of the dots in or out. She spoke of eye-kinaesthesis of contraction and expansion which paralleled this movement but which was never the basis for the judgment. She reported also 'flickers' of the dots which might be considered a movement but was not at all like the straight movement in and out.

II. Series II was not given to *D.*

III. In this series the larger stimuli were in every case judged solely on the basis of movement. Movement was also experienced with the comparison stimulus of the same size as the standard, and the judgment was made on this basis, except in one case, where the movement was recognized as false because it did not involve the dots, and the judgment 'equal' was consequently given. All other experiences where the judgment 'equal' was given involved some sort of movement, which was, however, always discounted. With the smallest comparison stimulus two judgments were made on relative position of the dots, and contrary to the direction of movement. Movement, all through the series, was, however, the criterion most frequently used.

IV. Distinction was made in this series between 'slightly greater,' 'greater,' and 'decidedly greater,' by the self-instruction of the *O*. The resulting judgments followed very closely the objective values. The higher values were judged 'greater' univocally by only one criterion, namely movement. Different kinds of movement were, however, distinguished. There were the simple movement, the 'jumpy' movement, and an actual 'jump.' The 'jump' did not appear below the largest comparison stimulus, nor the 'jumpy' movement below the next smaller. The judgments of 'equal' were made on the basis of absence of movement. Eye-kinaesthesis was reported throughout the series but was not used as a factor in determining the judgments.

V. The judgment 'less' was given on the smaller comparison stimuli in this series mostly in the presence of contraction, or of the 'jumping in' of the dots towards the centre. This was true also with the comparison stimulus equal to the standard. When this stimulus was judged 'equal,' movement had been experienced but was reported as unanalysable and did not seem to influence the judgment. In one case this stimulus was reported

'greater' because expansion of the dots had occurred after a contraction had already taken place. There were several cases where the larger comparison stimuli were judged 'equal' in spite of what seemed to be a contraction of the dots, and there was one case where this judgment was made in spite of both movement out and movement in. There were also several judgments of 'equal' made on the larger stimuli where *O* experienced no movement at all or only the unanalysable movement which she always discounted.

VI. Movement was the chief criterion in this series. Here all the comparison stimuli were smaller than the standard. With the smallest of these, where the illusion was present, the wings were seen performing a complete rotation, but with the larger stimuli they simply jumped from one position to another. Rotation of the wings without jumping from one position to another was reported where the illusion was overcome; the dots in these cases seemed to move towards the centre. The judgments of equal were made on the basis of rotation of the wings unaccompanied by expansion or contraction of the dots. Eye-kinaesthesis was experienced all through the series but never was made the basis of a judgment.

VII. Here the smallest comparison stimulus was just equal, subjectively, to the standard. All others were greater. With the smaller comparison stimuli, however, many cases of contraction of the dots were reported. Equality was judged on the basis of absence of expansion or contraction and the presence only of rotation of the wings or of rotation and unanalysed movement, which was never considered a genuine movement of the dots. This movement occurred also with the smaller stimuli when they were judged 'less,' even though they were objectively larger than the standard or equal to it; and very likely in these cases as well as in those cases where it was not reported this movement was responsible for the presence of the illusion. With the larger stimuli the wings seemed to change their position by jumping instead of by rotation, and the dots also seemed to jump. In several cases the dots gave the impression of movement outward after the rotation of the wings inward had taken place. Eye-kinaesthesis was reported throughout the series.

D's attitude was passive throughout. Her judgments were made wholly on the basis of contraction and expansion of the dots. This contraction and expansion took place in several different ways. Some movements were described as 'jumps,' some as 'jumpy movements,' others as simple movements. There was also an unanalysed movement which was, however, discounted in the making of the judgment. Rotation of the wings also occurred in the last two series. Most of the illusory judgments were followed by reports of 'pseudo-movement' as well as of contraction and expansion of the dots.

It is evident that the experience indicated by the judgments 'greater' and 'less' may sometimes accord with the objective displacement of the dots, and sometimes, under the influence of the *Nebenreize*, be in contradiction to the objective displacement. This fact is what is meant by the existence of an 'illusion.' The further problem lies in an understanding of the illusory displacement, *i.e.*, of the experience which contradicts the objective displacement.

It appears in the first place that although judgments according with the objective displacement and judgments of the experience contradicting it may appear identical there is, nevertheless, if the total consciousness be taken into account, a difference, or at least a tendency to difference, between the two types. The judgment which corresponds with the objective displace-

ment tends to be univocally determined, to issue from a number of criteria reënforcing one another. The illusory judgment, on the other hand, tends to be equivocally determined, to issue from a conflict of criteria. The conflict may be between the impression of extent and movement, between relative position of the dots and extent, between relative position and movement, or between movement and 'pseudo-movement.' Of course some of these criteria may be effective in 'correcting' the illusion, so that it frequently happens that a judgment in accord with the objective displacement issues from a conflict. Relative position of the dots seems most likely to counteract the illusion, whereas movement of the wings and 'pseudo-movement' almost invariably tend to enhance it.

Although the illusory experience tends to be characterized by conflict and non-illusory experience by univocal determination, exceptions occur in both ways. As we have seen, a conflict may be won in the direction that corresponds with objective displacement. On the other hand, there seem to be numerous cases of illusory determination that is univocal. Most noteworthy are the cases of judgments based upon simple movement which is seen in the direction opposite to the direction of displacement of the dots.

Table V shows the frequency of occurrence of such 'illusory' movements, *i.e.*, of movement reported in the direction opposite to the objective displacement ('1' of the Table) or movement in the direction of the illusion when there was no objective displacement of the dots at all ('2' of the Table). It will be noted that movement occurs much more frequently when it is not in opposition to the objective displacement. When there is opposition between the objective displacement and the subjective there is, of course, likely to be conflict of criteria, and it is natural that there should be fewer cases of reported movement where the conflict has been resolved in favor of the movement as a criterion than where there is less chance for conflict. There are also large individual differences in report. *Y* never reported movement, whereas *D* under the more favorable conditions experienced it in from 66% to 100% of all cases.

The question naturally arises as to the exact psychological nature of this reported movement. An objective displacement in one direction must be accompanied by a corresponding displacement in the image upon the retina. It seems almost incredible that a sensory experience of seen movement could take place in the direction opposite to the direction of shift in the retinal image. One is inclined to wonder whether the movement is after all actually seen as a process-experience or whether it may not be merely inferred on the basis of some static criterion. The answer seems to be that it is actually seen in sensory terms. It is certainly not a gross inference of the order of the stimulus-

TABLE V

Percentages of total number of illusory judgments which were based on movement of the dots as a criterion. *Y* did not report this criterion. A very few cases of movement contrary to the effect of the *Nebenreize* when there was no objective displacement of the dots are omitted.

Series	Percentages *B*	*H*	*D*	Total number of illusory judgments possible for each *O*
1. Movement reported as opposite to the direction of objective displacement.				
III	0	5	20	20
IV	0	5	20	20
V	0	0	0	20
VI	0	25[1]	24	50
VII	10	35[2]	22	40
2. Movement in the direction of the normal illusion reported when there was no objective displacement of the dots in either direction.				
III	10	33	100	10
IV	20	33	90	10
V	25	0	66	10
VI	—	—	—	0
VII	33	33[3]	100	10

[1]25, instead of 50, possible illusory judgments.
[2]20, instead of 40, possible illusory judgments.
[3]5, instead of 10, possible illusory judgments.

error, for the *O*s gave reports of process and fractionated the total experience into these various criteria of which movement was only one. The reported movement is therefore something more than a mere report of movement, than a bit of verbal behavior; movement is the name of a definite experience distinguished from displacement and change of extent. The bare fact that movement is thus separated off from the other criteria in itself tends to establish its status as sheer experience of the order of mental process. This indication is further supported by the expression of the *O*s who describe the movement as 'seen.' Ideally the final test of its sensory nature would be its equation either as meaning to the phi-phenomenon, or as process to the gray blur that Dimmick[11] showed to be the process-basis of the phi-phenomenon. The conditions of the experiment are, however, peculiarly unfavorable to the observation of the phi-phenomenon, and the *O*s moreover were untrained for such observation. The matter can therefore probably not be regarded as finally settled, but the indications are that purely illusory movement is sensory like ordinary movement and that under favorable conditions it would be found to reduce to the phi-phenomenon.

[11]F. L. Dimmick, *op. cit.*, 317-332.

Summary and Conclusions

1. According to the results obtained from our data the illusion is consistently less on the average when 'positive' than when 'negative.' (The illusion is positive when the *Nebenreize* tend to make the comparison stimulus relatively less than the standard; and conversely.)

2. The illusion was given about equally by the arrow-heads and by the feather-heads as *Nebenreize*, although the feathers were slightly more effective. The total illusion, however, when measured directly, was more than the sum of these two parts when measured separately, presumably because of the greater difficulty of keeping the dots of the figure focal with the more complex stimulus.

3. The illusion occurs strikingly (average = 16% of standard extent) even with short exposures and figures almost wholly in foveal vision, although under such conditions eye-movement is practically eliminated.

4. Illusory or paradoxical movement of the dots is in the single instance not different introspectively from movement in the direction of the objective displacement.

5. There are, in the critical region where the largest number of illusory judgments are given, many more cases of conflicts of judgment due to opposing criteria than in those regions where the effect of the *Nebenreize* is in the same direction as the objective displacement or is entirely counteracted by it.

6. The different kinds of criteria upon which the judgment may be based are (1) seen movement of the dots, (2) displacement of the dots without movement, (3) total extent between the dots, (4) eye-kinaesthesis of contraction and expansion, (5) movement of the wings and (6) 'pseudo-movement' (usually not definitely localized). The last two kinds of movement are usually not employed consciously but are reported to have been influential and to have played a part in many of the conflicts of judgment.

7. The presence of more than one criterion does not always mean opposition; sometimes it means reënforcement. Opposition comes chiefly in the critical region.

8. Movement is not invariably reported by any observer, although it is reported frequently, especially in the last two series. *Y* gives no reports of movement whatsoever.

9. The average amount of the illusion is 1.5 mm., or 16% of the standard length.

10. The illusion, in degrees of visual angle corresponding to the average amount of the illusion, is 1°14′. This displacement is about as great as the space occupied by one hundred retinal cones, or 50 cones at each end.

11. There is no obvious physiological explanation of the manner in which the seen *Nebenreize* produce a seen sensory movement without any corresponding displacement of the retinal image.

II. Relative Effects of the Total Impression and the Analytical Attitude

The 'critical' or 'analysing' attitude as distinct from the attitude of 'total impression,' under either of which it is possible to regard the Müller-Lyer figure and each of which gives different quantitative results, has been studied by Benussi[12] and by Koffka and Kenkel.[13] They have shown that the attitude of total impression is most likely to bring about the greatest amount of illusion; and also that anything in the stimulus which favors this attitude makes the illusion more likely to occur, and conversely that anything in the stimulus which favors the analytical attitude hinders the appearance of the illusion.

The object of the second part of this experiment was to discover, under certain conditions, the relative importance of each of these factors, the subjective and the objective, in bringing about the illusion.

The apparatus included a large square of white cardboard, 57 x 72 cm., secured to a solid wooden background of somewhat larger area. In the middle of this sheet of cardboard was a small horizontal slit about 5 cm. by 1 cm., behind which were the poles of a horse-shoe magnet. Each stimulus, a normal Müller-Lyer figure of cardboard, had attached to it on the back a darning needle by means of which the figure was held by the magnet against the slit in the cardboard. The arrangement gave the effect of a figure standing out clearly against a very large background and prevented judgment of the extent of the stimulus in relation to the extent of the background. The horizontal part of each figure was covered with velvet-black paper and the wings with one or the other of two different tints of gray (photometric values respectively equal to 67.5% and 26.7% of baryta white) or with velvet black (photometric value equal to 1.7% of baryta white). By the 'wings' is meant not only those parts of the figure which go off at angles from the main line but also the parts of the main line which would be included by perpendiculars dropped across it from the intersections made by the edges of the wings with the main line. Thus the main line extends only to the points of intersection with the wings, and is broken by the middle pair of wings. The standard part of this line was 5 cm. long and was bounded by arrow-heads. The comparison extent ranged in length from 4.5 to 5.5 cm. by intervals of .25 cm. for every one of the three kinds of stimulus (black wings, medium gray wings, and light gray wings).

There were, therefore, 15 different figures in all, which were presented in a haphazard manner in series of 15. *E* sat behind a screen to one side of the cardboard background and controlled the two 75-watt daylight lamps (type-C2 Mazda) just above the *O*'s head. The light was kept on until

[12] V. Benussi, *Arch. f. d. ges. Psychol.*, 1912, 24, 31-62; Referat über Koffka-Kenkel, *ibid.*, 1914, 32, 50-57.

[13] F. Kenkel, *op. cit.*; K. Koffka, Zur Grundlegung der Wahrnehmungspsychologie, *Beiträge zur Psychologie der Gestalt- u. Bewegungserlebnisse*, 1919, 203-282.

TABLE VI

Relative frequencies of judgment 'greater' (G) and 'less' (L) for Observers *P* and *M*, instructions I (total impression) and II (analytical attitude), and the three sets of stimuli, *i.e.*, light gray wings (L), medium gray wings (M), and black wings (B). These data are the observed psychometric functions from which the points of subjective equality of Table VII are interpolated.

	Instruction I					Instruction II				
		M		*P*			*M*		*P*	
	Comparison stimuli mm.	G	L	G	L	Comparison stimuli mm.	G	L	G	L
L	6.0	100	0			5.7	71	0	38	0
	5.5	82	0	100	0	5.5	13	0	7	17
	5.0	7	76	89	0	5.2	0	87	0	72
	4.5	1	93	24	1	5.0	0	87	0	98
	4.0	0	99	0	76	4.7	0	90	0	98
	3.5			0	100					
M	6.0	100	0			5.5	47	0	30	2
	5.5	99	1	10	0	5.2	7	23	0	58
	5.0	3	15	83	0	5.0	0	87	0	99
	4.5	1	65	46	1	4.7	0	94	0	98
	4.0	0	100	1	64	4.5	0	100	0	100
	3.5			0	99					
B	5.5	100	0			5.5	83	0	67	0
	5.0	78	2	99	0	5.2	17	1	7	17
	4.7			53	0	5.0	2	5	1	45
	4.5	0	73	23	2	4.7	0	93	0	100
	4.2			3	39	4.5	0	100	0	100
	4.0	0	99	0	70					
	3.5	0	100							

the *O* had made his judgment; it was then turned off and the next stimulus put in place. During the intervals between exposures the room was completely darkened except for a small shaded red light which *E* used in adjusting the next stimulus. The *O* was seated 1.9 m. away from the stimulus, which was on a level with his eyes and subtended an angle of about 3°.

Three *O*s took part in this experiment, but the final results were obtained from only two. These two were Dr. C. C. Pratt (*P*), who was very highly trained in introspection, and Miss E. F. Möller (*M*), who was only slightly trained in introspection. The third *O* was Dr. Halverson (*H*), who had already taken part in the experiments with the tachistoscope. The reasons for retaining only *P* and *M* will be discussed in connection with the introspective results.

I. *Instruction for attitude of total impression*: "When the stimulus is presented you are to pay attention to the total impression and to report on the length of the horizontal line at the left as compared with that of the horizontal line at the right."

II. *Instruction for analytical attitude*: "When the stimulus is presented you are to pay attention to the two horizontal linear extents, abstracting as far as possible from the 'wings,' and report on the length of the left extent as compared with the right."

Table VII is a comparison of the points of subjective equality obtained with each of the different tints of wings and with each of the instructions. Chart III is a graphic representation of the same data. The figures represent the points of subjective equality obtained from 100 judgments on every stimulus, determined graphically by linear interpolation between the observed frequencies.

TABLE VII

Points of subjective equality for observers *P* and *M*, instructions I and II, and stimuli L, M, and B (*cf.* legend to table VI). These points of subjective equality are the stimulus-values of the points of intersections of the psychometric functions for 'greater' and 'less' given in Table VI. They were determined graphically.

	Instruction I (total impression)			Instruction II (analytical attitude)		
	L	M	B	L	M	B
M	5.2	5.0	4.7	5.3	5.3	5.0
P	4.3	4.2	4.3	5.5	5.3	5.2

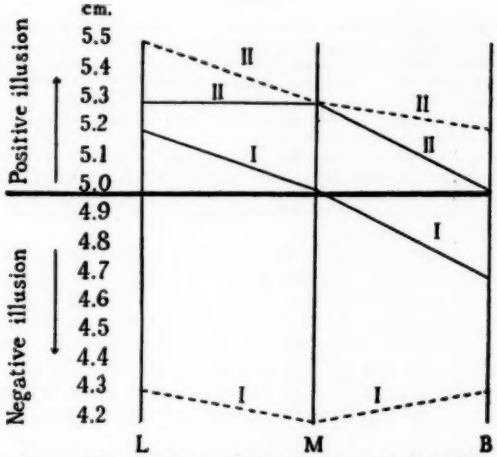
Both instructions (average)

	L	M	B
M	5.2	5.0	4.7
P	4.9	4.7	4.7

For *M*, L gives on an average about 2% less illusion than M;
M gives on an average about 2% less illusion than B.

For *P*, L gives on an average about 4% less illusion than M;
M gives on an average about 6% less illusion than B.

For *M*, Instruction II gives about 2% less illusion than I.
For *P*, Instruction II gives about 20% less illusion than I.



CHRTA III. Amounts of the illusion as indicated by points of subjective equality (Table VII) for *M* (solid line) and *P* (dotted line) for instructions I (total impression) and II (analytical attitude), and for stimuli with light gray wings (L), medium gray wings (M), and black wings (B). Since the standard stimulus is 5 cm., the illusion is positive when subjective equality is less than 5 cm., and conversely.

Summary of Introspections

Observer H, Instruction I. *H* experienced a great deal of eye-kinaesthesis and seen movement in the visual field, so that it was usually hard for him to come to a decision; when he did he was often quite dissatisfied with his judgment and felt that it might equally well have been for one of the other two categories. A special instruction was finally given him, limiting his judgment to his first total impression. This instruction, however, seemed to have little effect. It neither gave *O* more confidence, since he was always remembering that he had overlooked an impression previous to the one on which he had judged, nor did it seem to make any great difference to the objective results. The special instruction was given him partly because it was thought that it might reduce the number of 'equal' judgments of which there was an exceedingly large number. The frequency of these judgments seemed to accord with the fact that *O* took more time in reflection than did the other *O*s. The number of 'equal' judgments did decrease somewhat under the changed instruction, but at the same time the illusion continuously became very much larger, so that in order to use the method of constant stimuli a fresh start had to be taken frequently with certain stimuli discarded and other new ones added. Always there was much conflict and doubt whether a horizontal of 3 cm. was equal to one of 5 cm. or less than it, even when the stimuli with the light gray wings were used. It was therefore decided at this point that it would be more satisfactory not to try to use *H* in this particular experiment.

Some of *H*'s reports are interesting, however, because they show him to be an entirely different type of *O* from the other two. He reported a visual experience of movement, which took the form of a rapid, sometimes almost simultaneous, contraction-expansion process, although his experience

differed for the light gray, medium gray, and black winged figures. The comparison lines to which the light gray wings were attached seemed to expand only a very little, and most of their movement was contraction. The lines with medium gray wings seemed to expand in 'puffs,' although they became fixed when they were in focal vision. The lines with the black wings seemed to expand even when they were fixated, so that the illusion here was greater than in the case of the figures with medium gray wings as the illusion with the medium gray wings was in turn greater than with the light gray wings. These reports were actually borne out by the accompanying judgments with fair accuracy. Later on, all three kinds of figures gave for *H* about the same amount of illusion; but he evidently was not aware of this change; at least he failed to report any change in mental process, and continued to speak in general terms of the contraction-expansion process. In contrast with the other *O*s, he was always alive to his experiences, always reflecting on them. Since his quantitative results were constantly shifting, while those of the other *O*s were relatively stable, there may be a certain significant connection between such aggressive awareness of process and instability of objective judgments. All this sets him apart from the other *O*s who showed under both instructions a fairly rapid and thorough-going mechanization.

It might be added that during one experimental period *H* several times saw the figures in perspective and reported that the horizontal line on the over-estimated side of the figure looked like the end of a tunnel (the wings being receding walls), which came nearer or went farther away according as it seemed to expand or contract. The contraction and expansion seemed, however, to be the primary phenomena, so that this additional experience of depth could hardly be used in support of a perspective-theory.

H was not given Instruction II.

Observer M, Instruction I. *M* was at first extremely conscious of eye-movements, which started at the middle wings and went first to the left and then to the right. Her judgment was generally made on the basis of the length of the sweep of the eye-movement. *M* made her judgments more quickly as time went on, and experienced increasing difficulty in taking the figure as a whole. By the time the preliminary series was over and the experiment proper was begun, her judgment-process had become so mechanized that she found nothing more to say about it. She did not realize how she was making her judgments; they came automatically. At times she reported that it was difficult not to allow a knowledge of the illusion to influence her decisions and also that she was sometimes slightly prejudiced by the recognition of figures and the memory of previous judgments.

Instruction II. At the very beginning of this series *M* remarked that she thought she must have been abstracting away from the wings even under the instruction for total impression because she seemed to have no difficulty now in neglecting them. The only report of process after this time was a statement to the effect that 'equal' judgments, which were usually doubtful, were the only ones whose correlated conscious processes could be described.

Observer P, Instruction I. At first a certain amount of description of processes was given. The figures with the light gray wings were always hard to judge by total impression. The black horizontals persisted in standing out much more clearly and the *O* involuntarily squinted so as to exclude the wings altogether. If this did not succeed he usually closed and then opened his eyes and then made another attempt to get a true total impression. This total impression was characterized by a more nearly equal distribution of clearness than the impression most naturally aroused by the figures with gray wings, in which clearness was largely concentrated on the black linear extents. Little eye-movement was noted except at the very beginning.

Instruction II. A statement made just after the second instruction was given will suffice for the whole series. *P* was not aware afterwards that his attitude had undergone any change.

"The *Aufgabe* was quite conscious in the fore-period, sometimes a vocimotor image, or a horizontal or telescoped visual image of the lines; especially towards the end, it was present in a muscular set of the eyelids, in squinting. The judgment itself was fairly easy to make. Consciousness was much more specifically determined now than it was before, because now there was a narrowing down to one object. First there was very rapid narrowing of the visual field, culminating in a focal visual perception of a horizontal line at the left; then a rapid shift of focality to the right. Sometimes the judgment came quite automatically on the basis of this first experience. The following kinds of judgment are apt to be delayed: first, judgments where the two stimuli appear almost equal; second, judgments which depend on the sharpness of outline of the horizontal line. Sometimes, however, this seems to run imperceptibly into the wings. Then it is hard to get it focal. There are four degrees of difficulty: first, when all the figure is the same tint and the horizontal lines are nearly equal; second, when the wings are slightly different in tint all the way down to white (*O* evidently did not note the fact that there were only two tints of gray!) and the lines are the same length; third, when the wings are the same tint but the lines are very different in length; fourth, when the wings and the lines are both different."

Summary and Conclusions

1. It is obvious (Chart III) that the effect of a change of observational attitude dependent upon instruction may be of the same kind and magnitude as the effect of an objective change in the stimulus. The effect of the assumption by the observer of a critical or analytical attitude (Instruction II) instead of the attitude for total impression (Instruction I) decreases the illusion just as does a change in the tint of the wings in a direction to make them less similar in quality to the lines (from light to black wings when the lines are black). In noting this relationship in Chart III or Table VII, it must be kept in mind that the change in the illusion is superimposed upon a large constant space error, so that in most cases an increase in the illusion appears as a decrease of the negative illusion.

2. There is perhaps little reason to suppose that similar objective results necessarily imply similar psychological bases, but in this case it would appear that the changes due to the two causes (stimulus and instruction) depend upon like psychological mechanisms. One might expect in advance that qualitative similarity between the wings and the lines would conduce to a taking of the figure as a whole and that contrast between the wings and the lines would aid analytical observation. *P* actually does report this situation. With black wings upon a black line he was obliged to squint in assuming the analytical attitude, whereas it was natural for him to take the figure by total impression. Conversely the light wings naturally led him to analysis, and it was only with difficulty that he could include them in a total impression. *M* shows the same objective relations, though she gives no adequate account of the attitudinal basis.

3. There are differences between the two observers with respect to the effect of the tint of the wings. *P* under the instruction for total impression (I) shows no general effect of change of tint, and under the analytical attitude (II) a difference of 0.3 cm. for a change from light gray to black wings. *M* gets a difference of 0.5 cm. under Instruction I and of 0.3 cm. under Instruction II for the same change in the wings.

4. There are much larger individual differences in the effect of changed instruction. For *P* the changed observational attitude results in a shift of the point of subjective equality of 1.07 cm. on the average; whereas for *M* the corresponding shift is only 0.23 cm. on the average.

5. *P* is therefore more affected by the attitudinal change than by the change of stimulus, in the ratio 1.07/.15; whereas *M* is more affected by the change of stimulus than by the change of instruction, in the ratio .40/.23. This difference may in part be due to the fact that *M* did not radically alter her attitude when the instruction was altered for her; she reported under Instruction II that she was not even sure that she had changed her mode of observation since the series under Instruction I. That she did change slightly, the objective results taken in the light of *P*'s introspection would signify.

6. It will be seen that these results indicate the possibility of an objective measurement of attitudinal change. For either *M* or *P* separately it is possible to say what degree of photometric difference between the wings and lines should be equivalent to the attitudinal change involved in the change from one instruction to the other. This relatively indirect measurement of the effect of attitude has the advantage of being in kind and is more significant, therefore, than the more direct measurement in cm. of the shift of subjective equality. One induces by instruction attitudes that tend towards confluence of the figure into a whole or towards a separation of the figure into its analytical parts. Similarly one induces confluence or contrast objectively by reducing or increasing the qualitative similarity of the wings to the lines. Specifically in our two cases we may say that the analysis of the figure obtained by instruction to analyse was, in the case of *M*, half as great as the analysis obtained by the qualitative separation of the wings from the lines by a photometric distance equivalent to 65.8% of baryta white; and that, in the case of *P*, the degree of analysis induced by instruction was about seven times as great as was induced by the same photometric difference.

7. In general it seems reasonable to suppose, and our two cases support the supposition, that variability would be greater in the case of the induction of attitude by instruction than in the case of its induction by the objective conditions of the stimu-

lus. An instruction is notoriously uncertain in its effect; our results indicate that occasionally it might be abandoned in favor of some more objective and constant determiner.

General Conclusions

In the formulation of the present problem it was the intention to apply modern introspective technique for the accomplishment of the isolation of the bare sensory extent upon which judgment is supposed to be operating when the illusion is in force. It seemed, in advance, as if the illusion might readily turn out to lie entirely in an inadequate statement of the problem; as if a judgment, purporting to be a judgment of principal extents, might actually prove to be a judgment in part of the *Nebenreize*. A substitution of description of conscious content for the indeterminate judgment should test this assumption: in an adequate description it ought to appear at once whether the principal extents were changed in themselves by the *Nebenreize* or whether the change consisted merely in an extension of the judgment to include the *Nebenreize*. Benussi's demonstration[14] that the analytical attitude reduces the illusion tends to support the latter view, but it seemed prudent to press the matter further to the full description of the ultimate data.

The original programme, however, was thwarted by the discovery that it was impossible to reduce the consciousness to a bare simple experience of extent, to a mere process-attribute. With a relatively simple stimulus, an extremely rapid exposure, and an instruction focussed upon a single aspect, the essential data determining the judgment remained very complex as regards their structure. It was possible to push analysis far enough to render the component data univocal as regards judgment, that is to say, to obtain a pair of extensive data which meant 'greater' or 'less' without equivocality; but even such univocal determiners of judgment both actually and potentially are complex for description. To reduce experience to pure extensiveness proved impossible.

This result is undoubtedly as aspect of a much larger problem. The conscious ultimates as they enter into description are not after all bare unanalysable processes, but rather mental objects, occurring in a particular experiment and functioning under the determination of the problem for a given psychological meaning. We need not go into this question here, but simply point out that our failure to arrive at a simple extensive consciousness is consistent with Koffka's exposition of the psychological *Gegenstand*,[15] and is also in accord with certain experiments, as yet unpublished, by Dr. Yokoyama in the Clark Laboratory,

[14] V. Benussi, *Arch. f. d. ges. Psychol.*, 1912, 24, 31-62.

[15] K. Koffka, *op. cit.*, 249ff.

which indicate that the descriptive yield of introspection is in accordance with the psychologically objectifying determination under which the observation is made. Nevertheless the failure to obtain a simple extensive consciousness need not of itself have prevented the anticipated outcome. It would have been enough that simple mental data, even though given as mental objects, acted as univocal determiners of the extensive judgments and were individually subject to uniform description. We might still have expected that a judgment of the principal extents, from which the appearance of the *Nebenreize* was excluded as a basis, would have shown no illusion. We found, on the contrary, that the illusion was directly conditioned upon the principal extent as given visually. Paradoxical displacements and paradoxical movements of the dots directly determined the illusion. These displacements and movements were descriptively vivid as visual data. They were not inferred but 'seen.' They were caused by the presence of the *Nebenreize*, but existed as mental data in and of themselves and were the immediate basis of judgment, or else a discarded basis when some other criterion was preferred in a case of equivocal determination. The original hypothesis was therefore not borne out: the extents as seen do expand and contract by seen displacement or by seen movement of the delimiting dots. The judgment does not issue from the *Nebenreize* directly, but only indirectly as the *Nebenreize* determine the extent of the primary sensory impression.

Here then is a puzzle that awaits explanation. It is not unlike that old puzzle concerning the seat of the discrepancy between stimulus and judgment that is expressed by Weber's law, and we should naturally seek an answer in similar terms. Roughly speaking, this complex phenomenon may be separated into five successive phases: (1) the stimulus; (2) peripheral excitation or the retinal image; (3) central excitation, a vague region of the series where attitude and *Einstellung* may be supposed to get in their work, or where some hypothetical central form of nervous confluence might be imagined to be operating; (4) the primary mental datum, sufficiently simple in the given situation to act univocally as criterion for report; and (5) the judgment or report itself, not sharply differentiated from the primary datum but consequent upon it and sufficiently explicit for the descriptive record of the experiment. Between which of these terms does the illusion occur? Where does the discrepancy enter in?

It can not occur between the primary mental datum and the judgment. The illusion involves an alteration of the datum, not merely a change in the judgment of an unaltered datum. Thus the illusion is not 'psychological', as would have been said of such an explanation of Weber's law.

Does the change occur between the stimulus and the peripheral excitation? If the *Nebenreize* led to some sort of retinal confluence, so that the retinal image was shortened or lengthened over fifty cones at each end, then we might regard the illusion as situated here; but such an assumption seems highly improbable. Moreover, we should have then also to explain contrast at the level of the retina, since spatial contrast and confluence seem to be two extremes of the same phenomenon that conditions the Müller-Lyer illusion.

Accordingly we must interpolate the effect of the *Nebenreize* between the retina and the mental datum. It is of little use to inquire whether the interpolation is physiological, lying between peripheral and central excitation, or 'psychophysical,' lying between central excitation and the datum. The double-aspect view of psychology would preclude the latter hypothesis, and the former hypothesis is not explicit because there must be many levels of central excitation amongst which we can not distinguish. Somewhere here, however, one expects to find attitude effective; certainly attitude is not likely to be effective at the retina and we have shown (contrary to our own expectations) that is is not effective at the level of the judgment. It is in the central regions that Wertheimer[16] supposed the short-circuiting that explains seen movement to be, and movement is one of the criteria of the illusory change. The effect of qualitative difference in tint of the wings might perhaps be supposed to be retinal if there were any theory of retinal confluence ready to assimilate the facts; but it seems natural that this difference too is comparable or even, as has been argued above, similar to the attitudinal factor, and is effective only as it conduces to analytical observation or the reverse.

If we say that the fact of the Müller-Lyer illusion is a fact of confluence between the *Nebenreize* and the principal extent, we have to conclude further that it is neither an excitatory nor a judgmental affair, but that it exists as a presensational and post-excitatory confluence of central *locus*, dependent upon both stimulus and observational attitude for its realization.

[16]M. Wertheimer, *Z. f. Psychol.*, 1912, 61, 248.

AN EXPERIMENTAL STUDY OF THE PERCEPTION OF STICKINESS

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In a recent article[1] Cobbey and Sullivan call attention to the fact that relatively little experimental work has been undertaken by psychologists in the way of analysing the 'touch blends' into their ultimate elements. The present report of our attempt to isolate, analyse, and determine the essential conditions underlying the perception of *stickiness* is a response to this need. By perception of *sticky* or *stickiness*[2] we here mean such tactual experiences as are called forth when a portion of the skin to which a sticky object (*e.g.*, a pencil dipped in liquid glue) has been applied tends to adhere to, and is elevated by, that object during its removal.

A. Analysis of Stickiness

Our experimental work was carried on from October to May, 1921-22. All our *O*s were relatively untrained in cutaneous work; consequently the first two or three months were consumed in training them in general tactual observation and in striking upon a suitable method of attack for our particular problem. The seven *O*s were: Professor H. C. McComas, Mc; Miss Margaret Macdonald, M, a graduate of Vassar, now employed by the Psychological Review Company; Mr. C. H. Rice, R, graduate student; Mr. M. G. Scheck, S, assistant in psychology; Mr. J. D. Jackson, J, a trained psychologist connected with the New Jersey Department of Institutions and Agencies at Trenton; Mrs. M. J. Zigler, Z; and the writer, Zi. R was unable to complete the work, but finished most of the analytical part. Mc did not observe during the first three months, but was able to complete the work in a briefer period because of training. J was used in the last experiment only, that of synthesis, and knew nothing of the earlier work, not even that the experiment was running in the laboratory. He therefore served

[1] L. W. Cobbey and A. H. Sullivan, An Experimental Study of the Perception of Oiliness, this Journal, 33, 1922, 121ff.

[2] The term *sticky* is not wholly unequivocal, since we colloquially speak of a 'sticky' day in referring to a warm and humid atmospheric condition. A usage more likely confusable with that here intended is the use of *sticky* in the sense of a 'sticky' thistle or needle. While there doubtless are common elements in these two kinds of tactual *sticky*, the predominance of the pain quality in the case of the thistle or needle is sufficient to distinguish it from 'sticky' in our sense.

as a valuable check against any possible suggestion which the other *O*s might have incurred from the previous work. The writer served as *E* excepting when he observed, when Z acted as *E*; thus, Z knew something regarding the procedure at every step of the experimentation, but in all other respects she worked in ignorance; Zi wrote his own introspective reports, so as to avoid suggestion to Z.

(1) In our earliest work we used the very simple procedure of immersing the rubber end of a lead-pencil, the cone-shaped tip of which was cut off up near the point where the rubber meets the wood (here it measured 5 mm. in diam.), in sticky substances such as liquid glue, prunes, molasses, jelly, etc., and of applying the immersed end by a gradual downward freehand movement to the volar surface of the tip of *O*'s index finger or to the back of his hand between the third and fifth metacarpals; after allowing the stimulus to rest firmly on the skin about 3 sec. we removed it by a gradual freehand upward movement. Excepting a brief series of presentations in which the parts stimulated were active, the member stimulated was kept passive during the experimentation. 20 to 30 stimulations were made at each sitting, and the application was made at approximately the same place on succeeding presentations so as to avoid distraction. *O* was both blindfolded and screened from the hand which was stimulated, and was instructed that the stimulus would set up a tactual perception, which he should name and describe as fully as possible, giving all the sensory attributes that the experience exhibits.

From the beginning all *O*s distinguished between the pressure complex which was set up while the stimulus was being applied to and rested on the finger or hand, and the complex experience which was evoked by the adherence of the skin to the stimulus and by the separation of the skin from the stimulus during the removal. All *O*s reported that stickiness was the most prominent component in the latter complex, and asserted that this was aroused only during the removal of the stimulus. As the *O*s convinced themselves of this fact they were instructed to confine their descriptions to the experiences which were set up during the removal of the stimulus, unless those of the earlier phase of the stimulation had a direct bearing or threw light upon the experiences in the latter phase. We thus hoped to simplify the *O*'s task and to secure a fuller account of the experiences occasioned during the removal.

Under these instructions the *O*s designated the experience stickiness' from the outset; but sooner or later they began to distinguish between a 'pull' or 'pulling' or 'traction,' and a 'breakaway' or 'breaking loose' or 'tearing loose' or 'superficial release.' The temporal distinction between these two aspects of

the total experience was not clean-cut; as a rule the 'pull' or 'traction,' which usually involved the entire area occupied by the stimulus and often extended outward into the adjoining parts somewhat, preceded the 'breakaway' phase, although the latter set in before the former had subsided. Sometimes the 'pull' was most intense at the periphery of the stimulated area during the initial stages, and the point of maximal intensity shifted toward the center during the course of the experience; in other instances the 'pull' was uniformly intense over the entire area, the peripheral parts releasing or breaking away during the course of the experience. In either case the 'breakaway' phase set in in the peripheral parts of the area before the pulling at a slightly more central region had fully transpired; thus the two components, the 'pull' and the 'breakaway,' overlapped temporally.

The 'pull' was described as a deep-seated experience, whose intrinsic quality was dull pressure. During the earlier work there were rather frequent reports of strains, faint aches, and pains, as constituting a rôle in the 'pull' aspect, but as the *O*s progressed in training these were designated as irregular and inessential features of the experience. The 'breakaway' component was described as purely superficial, and of a light pressure or contact quality. Ticklish pressure and ticklish contact were often reported, but the ticklish characteristic was not invariably present. At this stage of the work all *O*s were inclined to regard both the 'pull' and the 'breakaway' as intrinsic and indispensable components of the sticky experience.

Different kinds of stickiness were also reported from the very beginning, the labels given being *smooth*, *rough*, *tough*, *dense*, *brittle*, *dry*, and *wet* or *moist*. None of the *O*s reported all of these qualities, but every *O* gave at least 4 different kinds.

(2) A brief series of experiments was carried out with two *O*s, Z and Zi, with the view of determining whether the perception of stickiness shows any essential change when the member stimulated is active. A bit of sticky substance was applied to the tip of the index finger, *O* being instructed to bring the thumb in firm contact with the tip of the finger and then to separate the two digits. Under these conditions stickiness manifested no new characteristics. Here also the experience of stickiness was occasioned only during the phase of separation; hence we are able to conclude from both of the foregoing experiments that this perception is called forth by a moving stimulus only.

(3) The procedure employed in the foregoing work was very crude, and permitted only the roughest sort of analysis. In order better to control the conditions of our experimentation and to simplify *O*'s task we built a piece of apparatus which we shall designate by the name 'pull gauge.'

This was built on the principle of an ordinary spring-balance scale. It consisted of a hollow, thin-walled brass cylinder, 20 cm. long and 12 mm. in diam. The upper end of the cylinder was closed by a metal cap. A small coiled spring was suspended inside the cylinder by being soldered to a short metal rod, fastened to the center of the metal cap. A longer small metal rod was attached to the lower end of the suspended spring and protruded through a small hole in the center of another metal cap, which closed the lower opening of the hollow cylinder. This small rod bore a flat metal disc of 12 mm. diam. on its lower end, just outside the cylinder cap, and the spring was adjusted so that it held the metal disc firmly up against the cylinder cap. A slit cut lengthwise in the cylinder wall permitted an indicator, which rode on the second metal rod at the upper end where it joined the spring, to protrude through the cylinder wall and to move downward as far as the spring could be stretched. The instrument was graduated along the margin of the slit in intervals of 5 gr. and was capable of measuring pulls as high as 80 gr. By spreading a bit of glue on the bottom surface of the metal disc and applying it to *O*'s finger tip, the strength of the adherence during the removal of the stimulus was registered by the extent to which the spring was stretched downward. Thus *E* was able to read off directly the amount of pull exerted upon the skin in units of 5 gr. at every stimulation.

One limitation of this instrument consisted in the fact that we were unable to determine beforehand the strength that the pull would register. Thus, when we desired to give a series of weak stimulations, it could not be predicted beforehand that the pulls would be 5 gr. or less, as we desired; occasionally a 10 or a 15 gr. pull resulted.

To improve the manner of applying the stimulus, the pull gauge was mounted vertically on the ends of two parallel horizontal brass rods, which were hinged at the other end to a vertical baseboard. The parallel rods were connected by a brass spring, which was so adjusted that it held the pull gauge at a position of rest 15 cm. higher than the position taken when the parallel rods were in the horizontal plane, the position assumed when the stimulus was applied to *O*'s finger. By this means the instrument was applied at a uniform rate by pressing downward gently on the upper one of the parallel rods, thereby stretching the spring connecting them. A slow and regular removal was effected by the elastic recoil of the spring as *E* gradually released the pressure on the rod.

With this improvement in the method of procedure the *O*s confirmed their earlier distinction between the deeper seated pressure complex, which was variously termed a 'pull,' 'pulling' or 'traction,' and the series of superficial contacts or light pressures, which was designated the 'breakaway' phase. Moreover, all *O*s now identified stickiness as belonging to the 'breakaway' phase, several stating explicitly that the pull serves merely as a cue suggesting that stickiness is coming, others merely remarking that stickiness is not aroused until the 'breakaway' phase sets in. The following are samples of the reports upon which the foregoing generalizations are based.

"The stickiness is at its best and is perceived as sticky at the moment when the skin begins separating from the stimulus" (Mc). "The pull made me expect stickiness, but it was not really sticky until the breakaway came" (M). "When the pull comes I think it is going to be sticky. The pull seems to act as a cue as to what is going to happen. Then when the breakaway comes I know it is sticky" (M). "Stickiness was not perceived until toward the end of the experience when the breakaway came" (Z).

"Stickiness is confined to the series of light touches that are felt just as the stimulus and skin separate" (R). "The sticky comes when the skin is released from the stimulus" (S). "The release gives the stickiness as it breaks away in patches" (S).

The quality of stickiness was again described as a series of light pressures or contacts, which often, though not invariably, have a ticklish or tingling character. The series of light pressures resulted from our use of an areal stimulus; the 'break-away' of the areal stimulus, which set up the light pressure complex, took place progressively from the peripheral parts of the stimulated area toward the center giving a series of 'break-aways' that were temporally closely connected. Simple 'sticky' seems to be made up of a field of light pressures, whose intensive, durative and clearness attributes show characteristic modifications. The light pressures first become clearer or more prominent and more intense, waxing to a moderate and sometimes to a fairly high intensity, from which there is a rather sudden drop to a low or zero intensity. The duration of the pressure quality in stickiness is sustained much longer than in the experience of simple pressure. These attributive modifications characterize the superficial cutaneous part of the experience and do not pertain to the deeper seated 'pull' experiences. The following sample reports indicate the general nature of these attributive changes.

"One strand of sticky remained long that time. It seems that the basis of the experience is a series of contacts, which fluctuate in intensity. The contacts increased in intensity a little and then decreased rather suddenly. The phase of increase was at least two or three times as long as the decrease" (R). "Three or four light contacts held on and were long drawn out and fluctuated in intensity as usual, increasing a little and then decreasing. That was the essence of the stickiness" (R). "The experience lasts much longer than when the stimulus is removed without giving sticky" (M). "The intensity of the series of contacts in the breakaway changes. This time it changed very gradually and while one was dying out another was getting stronger" (M). "Sticky came from a group of individual points, which were stimulated irregularly. There was a rather marked lessening of pressure as it passed from one to another" (S). "After the pull had well subsided the feeling of sticky came in. It was like a series of touches in which the intensity came into prominence more noticeably and then dropped out rather quickly" (S). "The sticky has the quality of pressure, it can't be anything else. Only the pressure lasts a little while longer than in mere touch and has a continuous change. It changes in intensity and in vividness." (Mc). "Stickiness seems to end rather suddenly. It seems to have a rather high intensity and then goes out rather suddenly" (Mc). "It was just as if a light feather brush moved over the finger and the skin adhered to it a little as it passed along. This adherence was right on the skin surface and was distinct from the *pull*. It was just a patch of light contacts and as it passed from one to another it gave a smooth sliding sticky" (Z). "The characteristic feature is the continuous change in intensity. A little area comes into clearness, increases in clearness a little and also increases in intensity a brief time and then dies out rather gradually. Usually the phase of dying out is rather brief but this time it was rather gradual" (Zi). "The most significant thing is the sustained dura-

tion during which the individual contacts become significantly clearer and gradually more and more intense and then suddenly drop out from a moderate intensity and high clearness to practical obscurity. This all took place within a very brief interval of time" (Zi).

The *Os* also began reporting certain criteria, which characterized the different qualities of stickiness. Almost invariably a cold or cool component was reported in the *wet* or *moist* sticky, and there was uniform stimulation over the whole area occupied by the stimulus; that is, there were no unstimulated spots in the pattern. *Rough* sticky consisted of a pattern of scattered or discrete sticky spots, among which intervening unstimulated areas were irregularly distributed. These discrete spots of sticky were described as being of considerable size and of patch-like pattern and were stimulated in rapid succession as the stimulus separated from the finger. *Smooth* sticky, as contrasted with *rough*, occurs when the breakaway of the stimulus gives a series of tiny, closely compacted sticky stimulations. It was designated as a massed pattern; the cluster of points was closely grouped and the points themselves contiguous. In this regard *smooth* was like *wet* or *moist* sticky, although it lacked the cold component which is distinctive of the latter variety. *Dense* sticky also is composed of a closely aggregated field of sticky points; it differs from the *smooth* in that it practically always occurs at high intensities. *Tough* sticky occurs at high intensities also; the distinguishing feature here is the very gradual decrease in the intensive phase during the breakaway. It is sometimes massed like the *wet*, *dense* and *smooth* varieties; at other times it has the scattered or discrete pattern of the *rough* variety. *Dry* sticky was reported very frequently, but no positive distinguishing criteria were found. It was negatively differentiated from *wet* or *moist* sticky in that it lacked cold, and it usually had the scattered or discrete pattern. It was reported very often in connection with *rough* sticky, and perhaps has no true sensory basis.

These different kinds of stickiness do not always occur separately; very often two varieties were assigned to the same experience. Thus, a sticky experience may be both moist and smooth, dry and rough, tough and dry, etc. Certain combinations occur together very frequently, other combinations never occur.

There are some indications that visual association plays a secondary rôle in distinguishing the various kinds of stickiness. Thus, *tough* sticky was reported as being made up of strands, strings or cords of glue or gum, *dry* of a group of tiny threads of dry glue, *rough* of irregular patches of sticky substance, *smooth* of slippery and spongy substances, and the *dense* variety betrays by its name that the massed pattern was in all likelihood partially, if not largely, visually represented.

Two *O*s made occasional reports that a good sticky experience resulted when the preceding pull was so weak as to be imperceptible and when there was no perceptible elevation of the skin. Consequently we gave a series of weak stimulations, in so far as our procedure permitted us to regulate the intensities of pull, in order to work out this point. The *O*s reported that good sticky experiences are set up at such low intensities of pull as 2.5 gr., when neither the pull nor the elevation of the skin is perceived. True, movement or elevation of the skin is usually perceptible in the stronger sticky stimulations; but all *O*s reported more or less frequently and more or less positively that the perception of elevation of the skin is not intrinsically essential to the perception of stickiness. A corroboration is found in the relatively infrequent mention of movement or elevation of skin in the reports, although the *O*s were instructed to give a full report of all sensory components of the experience. In experiments (1) and (2) we found that stickiness is realized only when the stimulus is moving; hence it appears that movement of stimulus is essential, while conscious representation of movement is not an indispensable condition of the perception.

(4) We next took a series of observations under the same experimental conditions excepting that a punctiform was substituted for the areal stimulus. A pointed needle was dipped in a sticky substance and applied to the finger. This procedure simplified the conditions of observation remarkably, in that *O* was permitted to give attention to the temporal course of a single adhering point in its simplest form; with the areal stimulation, all stages of stickiness were realized at any particular moment during the stimulation, since some points of stickiness were dying out at the same instant that others were coming into more pronounced vividness; and it was only by a process of careful abstraction that *O* arrived at the true temporal course of simple stickiness.

The simplified conditions enabled several *O*s, who hitherto seemed somewhat undecided as to certain features of the perception, to confirm more conclusively the true nature of stickiness. Owing to the fact that only one small point of adherence was perceived under these conditions, stickiness seemed to have lost something of its intrinsic quality according to the earliest reports; but all *O*s soon reported that this loss was illusory, and was occasioned by the very weak intensity and especially by the abbreviation of the temporal course. If the single punctiform contact exhibited a slight increase in clearness and in intensity and then decreased, it furnished a good sticky experience despite the fact that the intensity was low and the temporal course abbreviated. A good stickiness was not realized, however, at

every stimulation. In the unsticky instances the *O*s reported a touch in which there was no increase in clearness and intensity before the experience terminated.

(5) We next attempted to give a pull on the skin without setting up the conditions underlying stickiness; that is, we desired to get a method of lifting the skin without permitting the stimulus to break loose or separate from it. Our *O*s had reported that 'pull' is a separate and distinct experience, having no essential connection with stickiness, and that the latter perception is aroused when neither pull nor movement of the skin is perceived. By arranging conditions so that a pull on the skin could be made without allowing the conditions of stickiness, the breakaway, to take place, we hoped to establish these facts more conclusively.

The unmucilaged side of strips of passe-partout was glued to strips of cardboard, which were then cut up into 1.5 cm. squares. Several of these squares were stuck on the unhaired portions of *O*'s wrist and forearm. These 'stickers' were lifted by short pieces of thread, which were attached at the sides of the squares and joined above, care being taken to avoid lifting hard enough to cause them to break loose from the skin. Thus a pull on the skin was occasioned without involving the breakaway phenomenon.

Under these conditions we always got reports of the deeper seated pressure complex. Stickiness was reported in one or two instances only, when a small portion of the 'sticker' broke loose. In such cases *O* always described the experience as the characteristic superficial light pressure phenomenon, which was qualitatively distinct from the pull complex.

During the earlier part of this work several *O*s made occasional reports that they were unable to judge whether the experience, which we have designated as 'pull', was occasioned by a lift upward or by a pressure downward upon the skin.[3] Such reports appeared rather frequently in the case of two *O*s. In the work with the 'stickers' just reported all *O*s reported such things as: "That seemed just like a pressure downward upon the skin," and "That might have been a pull or a pressure downward, I am uncertain which it was." This mistaking a pull for a pressure seemed interesting enough to warrant more investigation; consequently we arranged a series of 40 stimulations, using the passe-partout stickers, in which one half were actuated by lifting the threads and the other half by a gentle pressure downward upon the 'sticker; *E* trained himself so that he was able to give approximately the same amount of pressure downward as was occasioned when the threads were lifted upward.

[3]Cf. E. B. Titchener, *Textbook of Psychology*, 147, 156.

The results show that pressure is mistaken for pull upward in 30 to 50% of the stimulations, and pulls are confused with pressures in 40 to 55% of the cases when weak stimulations are employed. Moreover, in many instances when *O* judged correctly, he qualified his judgment by saying he was uncertain whether it was a pressure or a pull, or that it might have been either. Besides, in some of his most positive judgments *O* was flatly wrong. These generalizations pertain in particular to the weaker stimulations; such confusions occur much less frequently at higher intensities of stimulation. The two intensities of stimulation used were 5 gr. = .022 gr./mm. and 20 gr. = .088 gr./mm. The following table embodies the result of our work.

	PULL JUDGED AS PRESSURE		PRESSURE JUDGED AS PULL	
O	Weak stim.	Higher stim.	Weak stim.	Higher stim.
M	40%	10%	50%	5%
Mc	50%	15%	30%	5%
S	55%	15%	50%	5%
Z	40%	10%	40%	10%
Zi	50%	10%	45%	10%

The reports indicate that certain sensory criteria aided in distinguishing between the pulls and the pressures. The pulls tended to extend out into the adjoining area; that is, the pull was not confined to the area covered by the 'sticker'; cords or threads of pull extended out into the adjacent parts. The pressures, however, were confined to the area covered by the stimulus, and there was often a sharp delimitation between this area and the adjacent region because of the sharper *pressure gradient* along the edges of the 'sticker.' Pressures are also characterized by a uniform firmness and compactness, while the pulls tend to lack uniformity of stimulation over the stimulated area. These differentiating criteria were much more pronounced at the higher intensities, while at the lower intensities they were scarcely perceptible.

A course of training, which extended over some weeks, was undertaken with one *O*, Mc, in order to determine the influence of training upon this illusion. It appears that, in so far as improvement was possible, it consisted in increased familiarity with the distinguishing criteria mentioned above. With the weaker stimulations Mc reduced the number of confusions to about 25% of the judgments, while with the higher the illusion rarely occurred after the course of a couple of weeks' training.

As a complement to the foregoing experiment with the 'stickers,' we attempted to determine whether stickiness is ruled out by anaesthetizing the skin with ether. When the skin was rendered completely anaesthetic only the deeper seated pull experience was reported; stickiness was not reported by any of the *O*s, though they would certainly have reported it if present

because of their previous work. The numbing influence of the ether prevented us from making certain that the pull in this experiment was identical with the experience which we have named pull above; it appears, however, that the pull previously mentioned is a combination of cutaneous and subcutaneous pressure.

B. Synthesis of Stickiness

Having carried through the analysis of stickiness and having determined the intrinsic conditions under which the perception is called forth, we next sought a method of synthetizing the perception. Unlike Bentley's *liquidity*[4] and Sullivan's *oiliness*,[5] both of which are compounds of two separate tactual qualities, stickiness involves only a single cutaneous quality, the attributes of which show distinctive modifications. We are therefore unable to present such a clean-cut synthesis as those investigators were able to achieve. Nevertheless, we were able to evoke the characteristic sticky experience by means of a stimulus which was totally devoid of any sticky substance. When a small sharply pointed fish-hook or needle was carefully inserted under the layers of the epidermis of the tip of the forefinger and lifted upward slowly and lightly, and when the lifting was terminated rather suddenly, all *O*s, including J who observed in this phase of the experimentation only, reported stickiness in 50 to 70% of the stimulations. If the needle or fish-hook happened to break through the epidermis during the lifting phase of the stimulation, the stickiness was either not good or was not perceived as such at all, the experience giving rise to a rough and scratching effect. Sometimes a rough stickiness was reported when the stimulus broke through the layers of the epidermis in a gradual fashion. The best sticky experiences were realized when the stimulus did not break through, but when the skin was lifted gently and then allowed to drop to its natural position without removing the needle. This called forth the sustained light pressure complex with the characteristic increase in clearness and in intensity and the rather sudden decrease in intensity as the pressure was decreased.

Our attempt to synthetize the areal form of stimulation was still more successful. Ten fine-pointed needles were driven through a piece of cork at an angle of 45° so that the points extruded about 2 mm. They were irregularly spaced at intervals of about 3 mm. from one another. This cork was applied carefully to the fingertip so that the cluster of needle points caught the outer layers of the skin and lifted it upward lightly and gently; as the tension on the skin increased slightly, the

[4] M. Bentley, The Synthetic Experiment, this JOURNAL, 9, 1900, 414ff.
[5] *Op. cit.*

separate needle points began breaking through the skin in succession, thus giving a series of releases. The breaking through the skin did not disturb the *O*s so much with the areal arrangement, probably because the tension of other points, which were still under stimulation, modified the experience of the single point that was just then breaking through the skin, thus altering the rough and scratching effect, which attended the breaking through of the single punctiform stimulus. Here all *O*s, J included, reported stickiness in 80% or more of the stimulations. The experiences were described as belonging for the most part to the dry sticky variety. J reported a sticky like tar or asphalt in 85% of the presentations in a total of 20 stimulations. The following are typical reports.

"That gave a good dry sticky. There was light pull and some elevation of the skin and then the breakaway. It was the kind of sticky that you get when you put your finger on something that has a little library paste on it that has almost dried" (M). "There was lateral pull followed by stickiness. Most of these sticky stimulations have been dry, but that one seemed gummy" (Mc). "That sticky suggested the feeling you get when you step in tar or asphalt on a hot street and then touch your finger to your shoe sole" (J). "There was a slight lateral thrust and pull and then the breakaway. There were several successive releases which gave a dry stickiness" (S). "That was a strong dry sticky. The series of stickies came with quite noticeable intervals of time between them. It felt like glue that had almost dried, but which adhered tenaciously and broke away in little patches" (Z). "That was a typical dry sticky. It had all the usual characteristics of dry stickiness" (Zi).

Conclusion

(1) Stickiness is a simple tactual perception, which consists of a field of light contact or pressure points, whose attributes show determinate characteristics: the intensity of the light pressures or contacts show a gradual increase and then a rather sudden decrease to a low or zero level of intensity; the duration of every sticky point in the field is noticeably sustained; and the point of maximal clearness shifts constantly and rapidly among the different pressures or contacts constituting the pattern.

(2) The term stickiness, as generally used, includes the deeper seated pull as well as the superficial light contacts which are set up when the sticky stimulus separates from the skin; but on introspective analysis these two experiences are denoted as distinctly separate and independent of one another, although the two are usually aroused conjointly.

(3) Stickiness is aroused by a moving stimulus only. Move- is not an intrinsic conscious moment in the perception, although it is consciously represented in most of the stronger experiences.

(4) The different kinds of stickiness have their basis partly in differences in the attributive combinations making up the pattern, and partly in visual associations. The former factor seems to be the more pronounced basis for these qualitative distinctions of stickiness.

(5) The perception of stickiness shows no essential alteration whether the member stimulated is active or passive.

(6) A sticky stimulus applied to a portion of the skin that has been rendered anaesthetic by ether fails to arouse the perception of stickiness.

(7) A light areal elevation of the skin is mistaken for a light pressure, and conversely a light pressure is perceived as a lift on the skin, as frequently as 30-50 % of the stimulations. At higher intensities of pull or pressure these confusions occur much less frequently.

AN EXPERIMENTAL STUDY OF AUDITORY TIED-IMAGES

By FORREST L. DIMMICK, University of Michigan

It is a common observation that, in listening to a speaker whose enunciation is not distinct, one fills in the confused or inaudible parts of his discourse. To the similar phenomenon in vision Titchener[1] gives the name "Tied-Imagery"; and a recent study by Zigler[2] includes an experimental investigation of the visual tied-image. The problem which we have undertaken is to determine the nature of the supplementation that takes place in the case of an incomplete auditory perception.

The term "tied-image" first appeared in a note by Titchener[3] in 1914. A term which has a similar form in German—*gebunden*—and which designates much the same class of images as does Titchener's "tied-image" is used by Höffding,[4] but is translated into English by M. E. Lowndes as "implicate." In his classification of images Titchener places tied-images in a group by themselves. Other authors make use of examples of tied-images, without specifically naming them, to illustrate the alteration or correction of our experience under the perceptive attitude.[5]

Procedure I.—For simple auditory forms we chose the opening phrases of familiar tunes, *e.g.*, Auld Lang Syne, Annie Laurie, etc. These were played softly on a piano whose strings had been closely damped to prevent any after-ringing of the notes. The *O*s were instructed: "I am going to play a short familiar musical phrase. I want you to listen carefully and to describe your experience from the beginning of the phrase until I say 'Now'." The phrase was not played completely, but was stopped short by several notes. When the time that would have been required to complete the phrase had elapsed, *E* said "Now."

The same phrase was repeated 5 times; a second phrase was then chosen; and the procedure was repeated until all of the phrases had been presented 5 times to every *O*. *O* sat with his back to the piano, and at the preliminary signal closed his eyes until his report had been completed.

[1]E. B. Titchener, Beginner's Psychology, 1915, 75.

[2]M. J. Zigler, An Experimental Study of Visual Form, this JOURNAL, 1920, 273.

[3]E. B. Titchener, A Demonstration of "Tied-Images," this JOURNAL, XXV, 1914, 300.

[4]H. Höffding, Outlines of Psychology, trans. by Mary E. Lowndes, 1891, 124.

[5]*E. g.*, W. B. Pillsbury, Essentials of Psychology, 1920, 170ff.; J. Jastrow, Fact and Fable in Psychology, 1900, 279ff.

The five *O*s who served were: G, R, T, students in elementary psychology, J. F. Finlayson, instructor in psychology, and Dr. C. C. Dimmick.

Results.—In their early reports all *O*s were taken up with the perception of the musical phrase, or with ideational complexes aroused by it. T, *e.g.*, described the perception as a series of notes thus: "I heard a full long tone, then a shorter tone of the same pitch; then a rise of two whole tones or more and a long and a short note played at that pitch; then another smaller rise in pitch by two more notes." F said, when "America" was played: "I had a feeling of patriotism and a realization of the appropriateness of rising." Reports of this sort were given during the first two or three observation-hours.

The next step which all *O*s made was to note the incompleteness of the phrase, as for example: G "6 notes ascending by pairs; 3 more should have gone with them"; F "I was conscious of an annoyance that you stopped short." Immediately with the noting of the incompleteness of the phrases appeared tendencies to fill them in. F "I hear the next 3 notes of the phrase but they are less full, hollow." G "I heard 5 or 6 notes and expected one more; that is, I heard it without its being played." R "I heard the last 2 notes as plainly as the ones you played." T "I thought I heard the high note [which was not played] but not as loudly as the others."

The supplied notes were usually distinguished for the *O* by being less loud and less rich. It soon appeared that the *O* did not, however, distinguish accurately the supplied notes from the preceding played notes. All *O*s reported the number of notes supplied and the pitch of the first one of these. In a large number of cases the number that the *O* was aware of supplying was one less than the number actually omitted. The first supplied note thus seems to have been indistinguishable from the played notes. This conclusion is substantiated in the second procedure. The percent of such confusions was for F 8%, for G 10%, for R 12%, and for T 20%.

Procedure II (*a*).—Our next step was to make the incompleteness of the presentation fall within the phrase, rather than at the end, and to reduce it to a single note. The experimental conditions were the same as before, and the *O*s were instructed as follows: "I shall play a short, familiar musical phrase which will be incomplete. I want you to note whether you supply the omitted notes in imagery and, if you do, to describe them as completely as you can."

Results.—The reports under these conditions substantiate the results under Procedure I. With the exception of a very few cases, most of which were accredited at the time to a lapse of

attention, the *O*s always filled in the missing note. Confusions with the actually played notes occurred, but were not so frequent as with Procedure I. For G the notes following the break came in and cut off the supplied note. T compared the supplied note with the succeeding one, and was thus able to mark it off more surely.

Procedure II (*b*).—We further altered our procedure to obtain a systematic attributive description of the image. To the foregoing instructions (Procedure II) we added the direction to the *O* to report only on specified attributes or perceptive aspects. These were required in the following order: pitch and duration, intensity and clearness, "richness" and "tone-color." The judgments were made in terms of relative likeness to the perception. Every musical phrase was presented 5 times to every *O* for reports on each one of the attributive or perceptive aspects.

TABLE I

	Pitch			Intensity				Duration			
O	−	=	+	V−	−	=	+	V−	−	=	+
D	33	59	8	0	12	30	58	0	28	67	5
F	2	96	2	13	37	43	7	0	17	76	7
G	31	62	7	3	17	47	33	0	47	32	21
R	7	29	64	3	16	31	50	10	28	27	35
T	0	100	0	0	16	50	34	3	17	42	38

	Clearness			"Richness"			"Tone-Color"				Confusions
O	bkgr.	part foc.	foc.	V−	−	=	?	tone	noise	=	
D	0	5	95	0	22	78	16	54	8	22	0
F	13	27	60	7	89	4	0	4	2	94	4
G	2	27	71	0	44	56	0	60	12	28	5
R	0	35	65	3	67	30	5	45	29	21	3
T	1	20	79	4	66	30	8	28	39	25	6

Symbols: V− means 'very much less'; − means 'slightly less (lower)'; = means 'same'; + means 'slightly greater (higher)'; *bkgr.* means in the background of attention; *part foc.* means in focus of attention with other processes; *foc.* means dominant in attention; *tone*, means tone predominated; *noise* means noise predominated.

Values are in percentages of 70 cases for every *O*.

Results.—Table I shows the distribution of the reports under the several categories for all the *O*s. In its attributive aspects the supplied note approaches very closely to the presented notes. Its pitch is very close to the pitch of the omitted note,

though 2 *O*s tend to hear it often as a bit flat, and one usually sharps it. However, the sharping or flatting is always characterized as "slightly." In intensity, for all *O*s but one, one third or over of the supplied notes are fully as strong as the presented notes, and 80% or more are not more than slightly less intense than the presented notes. There are only a few cases in which the image was "very weak." F is an exception to this rule, in that his intensity-curve moved in the direction of weakness. The duration of the supplied note approximated that of the omitted note, varying "slightly" above and below. Only 2 *O*s, and these in only a few cases, reported a supplementation "very much" shorter than the omitted note. Clearness was generally maximal, *i.e.*, the supplementation was focal in the majority of cases, though sometimes other processes shared the focus.[6]

In its perceptive aspects the image varies more widely from the presented notes. For 3 *O*s (F,R,T) the "richness" of the image is markedly less. For only one *O* (F) is the "tone-color" or "make-up" of the image the same as that of the played notes. For 3 *O*s (D,G,R) the image was just tone, without the characteristic noises of the instrument; for T the noisy character of the notes predominated. Some *O*s noted further characteristics that could not be brought together under these headings. D calls the image "silvery," "shrill," "all tone," "without any thud," and at other times "dull," "lifeless." Other characterizations are "thin," "rough," "ringing," "singing," "blurred," all of which point to textural differences between the struck note and the imaged note. This discrimination rests upon no single attribute, but upon a peculiar collocation of attributes and upon the perceptive setting. The fact that the *O*s were in many cases unable to distinguish the imaged from the perceived notes indicates that they could have been deceived a great share of the time if we had so desired. We were interested, however, in the tied-image under conditions where it is perceived as an image. The cases of confusion give further indication of the sensation-like character of the auditory tied-image.

Conclusions.—In this investigation we have shown, then, (1) that incomplete auditory perceptions are to a great extent filled in by auditory imagery which we may call "tied-imagery"; (2) that auditory tied-images are probably common to most individuals, since 5 *O*s chosen at random experienced them unmistakably; and (3) that the auditory tied-images are, attributively, closely akin to the corresponding sensations and differentiated from them mainly perceptively.

[6]The effect of training is evident here. D was a trained *O* at the start. G and T were especially apt, and observed for a longer period than did the other 2 *O*s.

These facts agree closely with the corresponding phenomena in vision. The first two are the basis for the wide use in illustrations and advertisements of incomplete figures. The description of the auditory images agrees with Zigler's[7] description of visual tied-images, which he finds attributively closely like sensation and distinguished from sensation largely by their perceptive setting.

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[7] *Op. cit.*, 279.

SOME NEW APPARATUS

By Karl M. Dallenbach, Cornell University

I. A Kinohapt

A modification of Benussi's kinohapt[1] is shown in Figg. 1—4. The apparatus, which may be held in any standard, consists essentially of two parts: a mechanical adjuster, shown in Fig. 1 as viewed from above, and in Fig. 2 as viewed from the side; and a magnetically controlled pressure stimulator, shown in Figg. 2 and 3 as viewed from the side.

It is possible by means of the mechanical adjustment to bring the stimulus to any desired point, since the apparatus may be set in all three dimensions of space. Screw *A* moves the carriage *x*, and consequently the stimulus, in one horizontal dimension; screw *B* moves the carriage *y*, and consequently the stimulus, in the other horizontal direction; and screw *C* moves the stimulus vertically.

The rods d_1 and d_2 project through the carriage *y* and serve to keep the stimulator in a rigid perpendicular position. At the lower end of the rod d_1 a steel spring *e* is fastened by means of the nut *f*. At the free end of the spring a gutta-percha block *g* is secured by two screws h_1 and h_2. Through the center of this block a screw *i* is threaded, the upper end of which is connected to bar *j* by means of a pin-hinge k_1, and this bar is connected at the other end to the lever *L* by a second pin-hinge k_2. The lever *L* extends back to and through rod d_1, in which it is pivoted, and ends in an iron disc *m*. Directly above the disc is the soft iron core of the magnet *N*. This disc is covered with a piece of chamois leather so that the noise of the contact of the magnet and arm is eliminated. The short arm is on the magnet side. It is one-half the length of the long arm; consequently the force exerted by the magnet is reduced, but the length of the excursion of the long arm is increased. To enable the apparatus to be compactly assembled, the short lever is displaced downward by two right-angular turns.

The stimulus point is secured in a hole *p* bored through the far end of the gutta-percha block by screw *q*. Since the excursion of the stimulus point parallels that of the end of the long lever arm, and depends upon the distance between the end of the short lever arm at *m* and the magnet, it may be controlled by adjustment of the screw *i*. If the gutta-percha block is removed from the steel spring *e* by unloosing the screws h_1 and h_2, screw *i* may be raised or lowered by revolving the block. As the height of *i* is varied so also will the distance between arm *m* and the magnet be proportionally varied. If the screw *i* projects far above the block, the arm *m* will be widely separated from the magnet, and the excursion of the stimulus point (when the magnet is excited) will be correspondingly large; if the screw is down, the separation of the arm and magnet and the consequent excursion of the stimulus will be small.

The excitation of the magnet may be controlled by any timing apparatus which will control the making and the breaking of an electrical current. When the current is made,—the wires being connected to the binding posts o_1 and o_2,—the short arm of lever *L* is drawn up, and the long arm forced down, moving correspondingly the spring *e*, thus bringing the stimulus, which previously has been adjusted to the skin by the screws *A*,

[1] *Cf. Archiv. f. d. ges. Psych.*, 29, 1913, 386; Benussi most frequently used the name 'kinematohapt,' but he at times abbreviated it to 'kinohapt.' We prefer and have therefore adopted the shorter term.

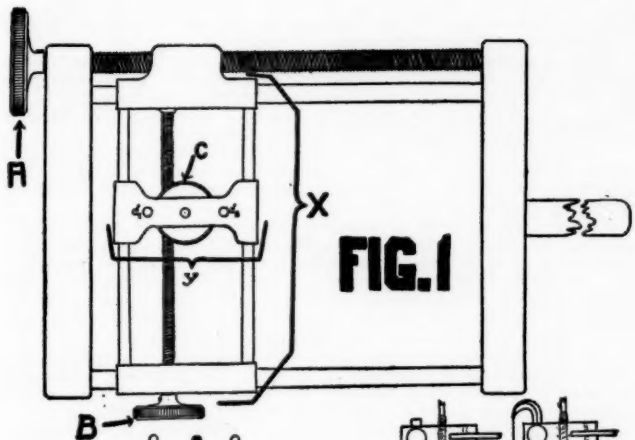


FIG. 1

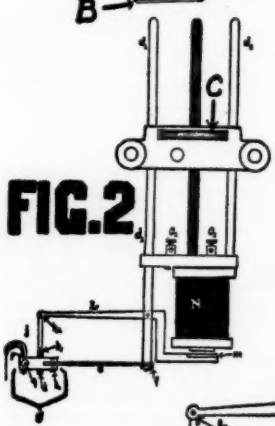


FIG. 2

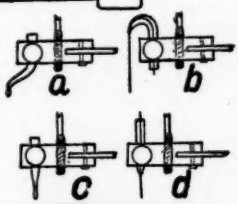


FIG. 4

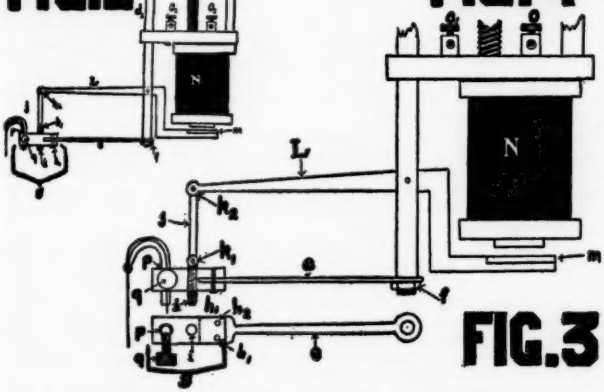


FIG. 3

B, and *C*, into contact with the point desired. As long as the current is made the stimulus will remain in contact. As soon as it is broken the magnet will be released and the lever arm and stimulus will be returned to their original positions by the tension of spring *e*.

The apparatus is adapted for sub-cutaneous as well as for cutaneous work; all that is necessary is a change of stimulus points. If sub-cutaneous stimulation is desired small wood, rubber or metal stimulators may be inserted in the holder at the end of the block; if cutaneous stimulation is preferred a hair aesthesiometer may be substituted.[2]

As many of these units may be used together as needed. The stimuli may be applied for any duration, in any sequence, and as often and at any rate or rhythm desired,—according to the arrangement and variability of the timing apparatus employed.

When isolated spots or widely separated areas are used, straight stimulus points such as are shown in Fig. 4, *c* and *d* are taken; but when neighboring spots or very small spatial intervals are desired, projecting stimulus points, such as are shown in Fig. 4, *a* and *b*, must be chosen. The shanks or butts of these points, 4 *a* for the subcutaneous and 4 *b* for the cutaneous, are so shaped that the stimulus points may, when two or more such units are employed in an investigation, be juxtaposed.

Sharp points may be substituted for the blunt, and the apparatus used in the investigations of the sensation of pain. The apparatus is therefore particularly adapted for punctiform stimulation in which control of the temporal moments and of the gradations of intensity is desired.

The apparatus has been used in a number of studies in this laboratory, the following of which have come to publication: A. K. Whitchurch, The Illusory Perception of Movement on the Skin, this JOURNAL, 32, 1921, 472-489; H. M. Lufkin, Cutaneous Localization and the "Attribute of Order," *ibid.*, 33, 1922, 128-135; W. A. Andrews, Haptical Illusions of Movement, *ibid.*, 33, 1922, 277-285.

II. A THERMAL STIMULATOR

A thermal stimulator, designed for punctiform stimulation, but equally well adapted for moving linear stimulation, is shown in outline in Fig. 5.

The stimulus-point *A*, a copper cylinder of 1 mm. diam. and 8 mm. length, pierces the point of the cone-shaped temperature chamber *B*. It projects 3 mm. beyond the end of the chamber, and (to increase conductivity) approximately 5 mm. within it. The temperature of the point is controlled by the temperature of the water flowing through the chamber. The stream is carried to *B* by tube *C*, and away by tubes D_1 and D_2. Tube *C* extends well within the housing, so that the current of water bathes the upper end of the stimulus-point.

The conducting tubes are shaped to form the frame-work for the support of the handle of the apparatus. The handle *F* moves freely upon the central tube *C*. It is made of gutta-percha, which serves to insulate the incoming water from the body heat of the fingers. The handle is tapered at its lower end in order that its position upon tube *C*, as registered on scale *H*, may be the more readily observed.

The pressure of the stimulus-point upon the skin is controlled, as in an improved Jastrow's aesthesiometer described by C. L. Friedline (this JOURNAL, 29, 1918, 402), by spring *G* which lies between the handle and rod *E*. Rod *E* not only serves as the upper carriage of the spring but also, by connecting the three tubes, serves to strengthen the apparatus. The weight of the stimulator is carried by the spring. Gradually, as the stimulus is applied by lowering the handle *F*, the weight is transferred to the skin.

[2]For aesthesiometers we used horse hairs which were fastened into small glass tubes by sealing wax and shortened or lengthened according to the intensity of the surface tension desired. *Cf.* Fig. 4, *b* and *d*.

FIG. 5

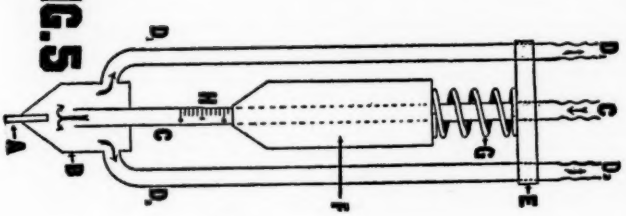
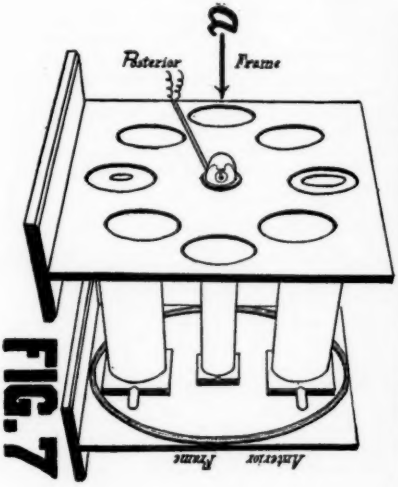
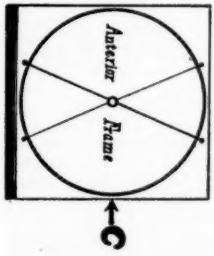


FIG. 7





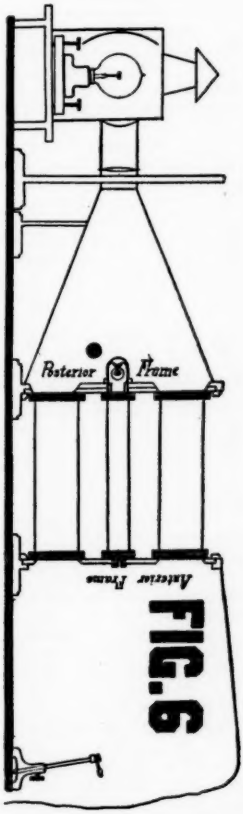


FIG. 6

The amount of pressure upon the skin is, therefore, within the limits of the weight of the apparatus, a function of the amount of the release of the spring,—a function of the distance that handle *F* is lowered on scale *H*. If a pressure greater than that of the weight of the apparatus is desired it may be obtained by placing small circular weights around *C* on rod *E*; if a pressure less than the weight of the instrument is desired it may be obtained and measured, as in the Griesbach aesthesiometer, by controlling the movement of the handle *F* on scale *H*.

The principal advantages of this apparatus are as follows:

(1) In the Willyoung, Kiesow, and von Frey thermaesthesiometers the rubber conduits are attached to the apparatus near the bottom; our use of the metal conduits as a framework for the support of the handle permits the transfer of the rubber tubes to the top where they are out of the way.

(2) The use of the weights and scale *H* do away with the necessity of employing springs of different tension (as in the Friedline instrument) for variable amounts of pressure stimulation.

(3) The stimulus may, if the cylinder is applied vertically and if the surfaces between the handle *F* and tube *C* are kept clean and well oiled, be applied at practically a constant pressure. If used punctiformly the pressure may, by controlling the position that handle *F* is lowered on scale *H*, be kept constant from experiment to experiment; and similarly, it may, if the apparatus be used by the continuous method, be kept constant from moment to moment.

(4) Stimulation by radiation, if not entirely eliminated, is reduced to a minimum; the housing of chamber *B* is small and by means of the projecting point *A* is removed from the skin. The short time that the point rests upon any single part of the skin, with either method of procedure, is not sufficient with this apparatus to stimulate the spots by radiation.

(5) The projecting point also permits of more accurate localization; the point of the stimulus is not obscured by the housing, so that the position of the point at the time of *O*'s report may be exactly recorded.

III. An Apparatus for the Study of the Conditions of Clearness

An apparatus for the study and equation of the conditions of clearness, attributive and cognitive, which has gradually been evolved in the Cornell Laboratory[3] is shown in its complete form in Figg. 6 and 7.

The apparatus, as shown in profile in Fig. 6, consists essentially of three parts: (1) a projection lantern with tachistoscopic attachment; (2) a diffusion chamber; and (3) an observational booth.

O sits in dark adaptation in a light-proof booth with his head held firmly in position by a biting board so adjusted that the fixation-point extended will fall midway between his two eyes. The biting board eliminates involuntary head movements and insures a constancy of position from experiment to experiment. The fixation point is 1 m. away from the eyes and 1 mm. in diam.

Two areas on opposite sides of the fixation point are simultaneously illuminated. The shape, size, position, quality and intensity of these areas may be varied concomitantly or independently. The shapes may, of course, be novel or familiar. The fixation point is held by the *O*, who reports after the exposure which of the two areas, differing from one another in one or more of these dimensions, is the more clear.

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[3] *Cf.* L. G. Meads, *Form vs Intensity as a Determinant of Attention*, this Journal, 26, 1915, 150; J. N. Curtis and W. S. Foster, *Size vs Intensity as a Determinant of Attention*, *ibid.*, 28, 1917, 293; A. M. Bowman, *Size vs Intensity as a Determinant of Attention*, *ibid.*, 31, 1920, 87.

Variation of the dimensions mentioned is made possible by what we have chosen to call the diffusion chamber. This chamber, the essential feature of the apparatus, consists, as is shown in outline in Fig. 6 and in detail in the drawings of Fig. 7, of two parallel screens and a rotating drum. The anterior screen, the one facing the *O*, Fig. 7c, is merely a frame which provides a hub and rim for the drum. The opposite screen, Fig. 7a, is solid except for 8 circular windows spaced 45° apart and at a uniform distance from the center. At the center there is a small circular opening which serves a dual purpose; it acts as a boxing for the posterior axle of the drum, and as a window through which the light from a small electric lamp placed in the housing before the hole is transmitted to the fixation point.

The drum, Fig. 7b, is composed of two parallel discs and three connecting tubes. There are three circular windows in each disc; a small one at the centers and two large ones at opposite edges. The holes in the two discs are opposed and connected by the three tubes. The small central tube is extended beyond each disc so that it forms the axle on which the drum is rotated.

At each end of every tube there are two plate or shutter holders. At the anterior ends, the ends near the *O*, ground glass plates are placed in the first holders, and metal shutters, which govern the size, shape, and quality of the exposed areas, are inserted, as desired, in the second holders. At the posterior ends, there is a second set of ground-glass plates which diffuse the light, and a second set of metal shutters which control the amount of light that falls upon these plates. The positions of the plates and shutters are shown in the drawings in Figg. 6 and 7; the ground-glass plates are represented by cross-hatchings and the shutters by straight lines. In Fig. 7a both the anterior and posterior shutters are shown in place; the anterior shutters as seen from the back as they project from the holder, and the posterior shutters as observed through the two windows in the posterior screen.

When the drum is placed in position in the frames the light from the lantern is transmitted through the two large tubes and illuminates the ground-glass plates fronting the *O*. The angular position of these areas to the fixation point is determined by the position of the drum, and this is controlled in relation to the windows in the posterior frame by a scale and marker which appears on the anterior side of the posterior frame and disc.

The illumination is derived from a 100-watt concentrated filament Mazda lamp placed in a projection lantern. The position of the lamp in respect to the lenses, and the focal distance of the lenses, are so adjusted that the disc of light projected upon the face of the diffusion screen is of uniform intensity over its entire surface.

Between the lantern and the funnel, which projects from the lantern to the diffusion chamber, is a gravity tachistoscope—an adaptation of Wundt's apparatus—which controls the duration of the exposure.

All the interior surfaces of the apparatus are enameled white so as to reduce the absorption of light to a minimum.

STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

XLV. Absolute Judgments of Character Traits in Self and in Others

By Margaret F. Washburn and Vlasta Stepanova

H. L. Hollingworth reports in Ch. VII of his "Vocational Psychology" the results of experiments made on Barnard students, in which members of groups of 25 persons arranged themselves in order of merit with reference to the following traits: neatness, intelligence, humor, conceit, beauty, vulgarity, snobbishness, refinement, sociability. The results related to the following problems: (1) the agreement of self-estimates with estimates by associates; (2) the existence of tendencies toward overestimation of a given trait in oneself; (3) the relation between possession of a trait and ability to judge it in others. In addition to the estimates, the results of certain mental tests were taken into consideration.

The method of the present Study differed in two important respects from Hollingworth's method. In the first place, our observers, instead of comparing the members of a group among each other and arranging them in order of merit, thus using a relative standard, made absolute judgments of the presence or absence of certain traits. In the second place, we used a set of traits in which the factor of complimentariness or uncomplimentariness was reduced to a minimum; Hollingworth's results showed that this factor was of decided influence. We selected our list from the traits used in the Downey Will Profile Test, as follows: careful—careless, ambitious—unambitious, punctual—tardy, industrious—not-industrious, enthusiastic—indifferent, self-confident—self-distrustful, impulsive—deliberate, orderly—disorderly, optimistic—pessimistic, patient—impatient, aggressive—unaggressive, suggestible—not-suggestible, thrifty—extravagant. Every member of a group was asked to judge herself and every other member of the group by checking one member of each pair of traits in the case of each person judged.

There were three groups of *Os*: two of these comprised ten persons each, who had been living in close proximity in a college dormitory and were friends and companions.[1] The experiment was performed on each of these groups in the following way. The members sat with paper and pencil, and each wrote the name of one of the group members: the list of traits was then read by the *E*, and each person noted down that trait of a pair which in her judgment characterized the individual she had chosen to judge. The process was repeated until every person had judged every member of the group, including herself, with reference to every pair of traits. The nine members of the third group did not perform the experiment simultaneously: the *E* went to them individually. They were probably not so intimately acquainted with one another as those in Groups I and II, as they had not actually lived together. This difference in the method used with Group III was not accompanied by any apparent difference in the results: as will be seen, Group I and III gave results more closely parallel than did Groups I and II.

Our results go to show first that, using this method of making absolute rather than comparative judgments, and judging with reference to the list of characteristics given above containing no very unfavorable traits, *our Os tended to judge others as they judged themselves.*

[1]The experiments on this group were performed by Miss Frances Rudy.

The evidence is as follows. In each of the three groups, the number of persons judging themselves to have a positive trait was counted. Each of these persons had judged a certain number of her companions to possess this positive trait. These latter numbers were added, giving the sum of all positive judgments on others made by the persons in that group who judged themselves positively. This sum was then divided by the number of such persons: the quotient represented the average number of times that an individual in the group who judged herself positively judged others positively. The same procedure was then followed in the case of the members of the group who had judged themselves to possess the corresponding negative trait; that is, the average number of times was found that an individual in the group who judged herself negatively judged others positively. Evidently, if this second average was lower than the first, there was a tendency for the members of the group to judge others as they judged themselves.

The figures were as follows.

Carefulness. Group I, average number of positive judgments on others: positive *Os* (self-judged), 8; negative *Os*, 6.3. Group II, positive *Os*, 6.6; negative *Os*, 6.7. Group III, positive *Os*, 8.2; negative *Os*, 6.4.

Ambition. Group I, positive *Os*, 7; negative *Os*, 4.7. Group II, positive *Os*, 6.5; negative *Os*, 5.5. Group III, positive *Os*, 8; negative *Os*, 6.

Punctuality. Group I, positive *Os*, 8.1; negative, 6.6. Group II, positive *Os*, 7; negative, 8. Group III, positive *Os*, 8; negative, 5.1.

Industry. Group I, positive *Os*, 9.1; negative, 7. Group II, positive *Os*, 7; negative, 7. Group III, positive *Os*, 7.7; negative, 6.5.

Enthusiasm. Group I, positive *Os*, 7.2; negative, 5.6. Group II, positive *Os*, 6.8; negative, 4.6. Group III, positive *Os*, 7.6; negative, 8 (only one case).

Self-confidence. Group I, positive *Os*, 6.5; negative, 5.1. Group II, positive *Os*, 6.5; negative, 6.1. Group III, positive *Os*, 4.8; negative, 5.5.

Impulsiveness. Group I, positive *Os*, 6.5; negative, 2.1. Group II, positive *Os*, 3.2; negative, 3.3. Group III, positive *Os*, 3.2; negative, 3.3.

Orderliness. Group I, positive *Os*, 9.5; negative, 5.5. Group II, positive *Os*, 7.7; negative, 6.5. Group III, positive *Os*, 7.5; negative, 4.8.

Optimism. Group I, positive *Os*, 6.7; negative, 7 (two cases). Group II, positive *Os*, 8: negative, 7.2. Group III, positive *Os*, 7.6; negative, no cases.

Patience. Group I, positive *Os*, 7.5; negative, 5. Group II, positive *Os*, 4.3; negative, 4.7. Group III, positive *Os*, 7; negative, 5.4.

Aggressiveness. Group I, positive *Os*, 5 (two cases); negative, 2.4. Group II, positive *Os*, 5.5 (two cases); negative, 4.2. Group III, positive *Os*, 5; negative, 1.6.

Suggestibility. Group I, positive *Os*, 6.1; negative, 5.5. Group II, positive *Os*, 6.3; negative, 4(one case). Group III, positive *Os*, 5.3; negative, 5 (two cases).

Thrift. Group I, positive *Os*, 9.5; negative, 4.7. Group II, positive *Os*, 7; negative, 6.8. Group III, positive *Os*, 7.3; negative, 5.6.

It will be noted that the tendency to judge others as we judge ourselves is less marked in Group II than in the other two groups. In Group II, in 5 out of the 13 traits, there is no indication of such a tendency. Group III fails to show it in the case of two traits; and Group I in the case of one trait.

There is probably no significance in the relative strength of the tendency in different traits as indicated by the figures: the number of cases is too small. The existence of a general tendency however seems apparent, and there seems to be also a tendency to avoid judging others as impulsive or aggressive, whether one judges oneself so or not.

Our data relate also to a second problem, namely: Is a person who judges herself to have a certain positive trait judged to possess it by the majority of her group? This is one of Hollingworth's problems: do we tend to overestimate or underestimate ourselves in relation to certain traits? Our method involves a comparision of ourselves not with other individuals, but with a certain mental standard, necessarily vague.

To answer this second question, we proceeded with the data as follows. Each person in a given group who had judged herself to possess a certain positive trait had been judged by a certain number of her companions to possess this trait. These numbers were added, giving the sum of all positive judgments made by others on the members of that group who had judged themselves positively. This sum was divided by the number of such members, and the quotient represented the average number of persons who had judged positively an individual in the group who had judged herself positively. The same procedure was followed in the case of those members of the group who had judged themselves negatively; that is, the average number of persons was found who had judged positively an individual in the group who had judged herself negatively. When these averages were higher than 5 (since the size of the groups was 10, 10, and 9 respectively), a majority judgment of possession of the positive trait was indicated. The figures follow.

Carefulness. Group I, average number of positive judgments received from others: positive *Os* (self-judged), 6.3; negative *Os*, 5.6. Group II, positive *Os*, 8.6; negative, 5.4. Group III, positive *Os*, 7.7 ; negative, 6.8.

Ambition. Group I, positive *Os*, 6.8; negative, 4. Group II, positive *Os*, 7.4; negative, 5. Group III, positive *Os*, 7.7; negative, 7.

Punctuality. Group I, positive *Os*, 7.7; negative, 7.6. Group II, positive *Os*, 8.8; negative, 3.6. Group III, positive *Os*, 6.6; negative, 5.8.

Industry. Group I, positive *Os*. 8.3; negative, 7.5. Group II, positive *Os*, 8; negative, 5.5. Group III, positive *Os*, 7.8; negative, 6.

Enthusiasm. Group I, positive *Os*, 6.4; negative, 6.4. Group II, positive *Os*, 8; negative, 3.3. Group III, positive *Os*, 7.7; negative, 7.

Self-confidence. Group I, positive *Os*, 7.5; negative, 4.8. Group II, positive *Os*, 9; negative, 5.4. Group III, positive *Os*, 5.6; negative, 4.

Impulsiveness. Group I, positive *Os*, 4; negative, 3.6. Group II, positive *Os*, 8; negative, 2.1. Group III, positive *Os*, 4; negative, 3.3.

Orderliness. Group I, positive *Os*, 7.2; negative, 6.8. Group II, positive *Os*, 7.2; negative, 6.1. Group III, positive *Os*, 7.2; negative, 5.

Optimism. Group I, positive *Os*, 6.5; negative, 8. Group II, positive *Os*, 8; negative, 7.4. Group III, positive *Os*, 6.9; negative, no cases.

Patience. Group I, positive *Os*, 7.5; negative, 6. Group II, positive *Os*, 8.3; negative, 3. Group III, positive *Os*, 6.7; negative, 5.6.

Aggressiveness. Group I, positive *Os*, 3; negative, 2.8. Group II, positive *Os*, 8.5; negative, 3.6. Group III, positive *Os*, 4.5; negative, 3.3.

Suggestibility. Group I, positive *Os*, 6,1; negative, 5.2. Group II, positive *Os*, 8.5; negative, 3. Group III, positive *Os*, 5.7; negative, 3.5.

Thrift. Group I, positive *Os*, 6.5; negative, 7. Group II, positive *Os*, 7; negative, 4.8. Group III, positive *Os*, 7.5; negative, 5.3.

It will be seen that our *Os* in all three groups underestimated their possession of the following traits, in the judgment of their companions: carefulness, industry, orderliness. Optimism would probably show the same tendency, but for the circumstances that no member of this group judged herself to be pessimistic. The members of Groups I and III underestimated their punctuality, their enthusiasm, their patience and their thrift. On the other hand there was a tendency in Groups I and III for the members to overestimate their own impulsiveness and aggressiveness.

Group II differed from the other two groups in the relatively high degree of unanimity among its *O*s. In Groups I and III there was not a single person as to whose lack of any positive trait her companions were unanimous. In Group II their were 15 such cases of unanimous negative judgments. Nobody thought *O* II of Group II thrifty nor *O* III impulsive or aggressive, nor *O* IV self-confident, impulsive, or aggressive, nor *O* IV patient, nor *O* VIII enthusiastic, impulsive, or suggestible, nor *O* IX careful, punctual, patient, or thrifty, nor *O* X self-confident. The case was analogous as regards unanimity of positive judgments. In Group I there were 6 such cases: everybody thought *O*s A and F punctual, *O*s A and H industrious, *O* A suggestible and thrifty. In Group III there were no cases of unanimously positive judgments. In Group II, on the other hand, there were no fewer than 27 cases in which all the judges agreed in ascribing a positive characteristic to an individual. *O* I was agreed to be ambitious, self-confident, optimistic, and thrifty; *O* II to be ambitious and enthusiastic; *O* III to be careful, punctual, orderly, and thrifty; *O* IV to be patient, suggestible, and thrifty; *O* V to be punctual and industrious; *O* VI to be careful, punctual, industrious, and orderly; *O* VII to be ambitious, self-confident, enthusiastic, and orderly; *O* IX to be enthusiastic, and impulsive; *O* X to be ambitious. This unanimity in Group II may have been due to either or both of the following reasons: its members were persons of more marked traits than those of Group I; they were accurate judges of one another's character.

Two important influences suggest themselves as affecting the relation between judgments of self and judgments of others on oneself when made thus 'absolutely'. The first relates to whether the trait judged is one that is habitually repressed in society. People do not give free social expression, as a rule, to their pessimism, their impatience, their impulsiveness, and their aggressiveness. Hence, as our results show, when judging themselves absolutely they regard themselves as more pessimistic, more impatient, more impulsive and more aggressive than their companions think them. Secondly, traits like carefulness, punctuality, industry, and orderliness, refer to an ideal standard, which will be higher in proportion as a person's performance judged by others is more excellent. In other words, a really careful, industrious, orderly, and punctual person is not satisfied with his own performance. Hence our results show a tendency to underestimate one's own possession of these traits when one's own judgments are compared with those of one's companions on oneself.

XLVI. A Study of Revived Emotions

By Margaret F. Washburn, Ruth Field and Elaine D. Wolf

The object of this Study was to gather data concerning the speed and intensity with which anger, fear, and joy can be revived under instructions to revive them, and the relation of these data to the remoteness in time of the original occurrence of the emotion. So far as we know, this is the first attempt to investigate such a problem. We also collected data regarding the bodily expressions of revived emotion.

Our *O*s, on whom the experiment was performed individually, were 90 young women college students. The following instructions were given them: "I want you to recall some occasion when you were very angry. Let the anger develop again as far as it will, so that as far as possible you relive the incident and its accompanying anger. Do not speak, but give a signal when you feel the anger as fully as you can, in this revived form."

A stop-watch was started as soon as the *O* understood the request and began to fulfill it, and was stopped when she gave the signal. The time

was recorded, together with observations on the *O*'s attitude and facial expression during the process of recall. The *O* was then asked the following questions, and her answers recorded.

"1. Did you feel, in recalling the anger, any sensations from your own body, such as those of quick breathing, flushing, stiffening of the muscles, and so forth? Recall the anger again, if necessary, to describe these sensations.

2. Is the recalled anger as intense as the original anger, somewhat less intense, very much less intense, or do you only remember that you were angry, without being angry again?

3. How long ago did the incident occur?

4. Indicate which of the following causes occasioned the anger: humiliation, injustice, cruelty, being interfered with. Was the injury to yourself or to another?" (The answers to this question are not considered in the present report).

After the *O* had answered these questions a similar procedure was followed for fear. The instructions were: "I want you to recall, silently, an occasion on which you were very much frightened. Recall it fully, and re-live, as far as possible, the fear, giving a signal when you have succeeded." The time was recorded as before, the facial expression and bodily attitude noted, and the answers to the following questions.

"1. Did you feel, in recalling the fear, bodily sensations? Recall it again if necessary to describe those sensations.

2. Is the recalled fear as intense as the original fear, somewhat less intense, very much less intense, or do you only remember that you were frightened?

3. How long ago did the incident occur?"

Thirdly, a similar procedure was followed for joy. The instructions were: "Now recall, silently, an occasion when you felt great joy. Recall it fully and re-live, so far as possible, the joy, giving a signal when you have succeeded."

The time of recall was recorded, the facial expression and bodily attitude noted, and the answers of the *O* to the following questions.

"1. Did you feel actual bodily sensations in recalling the joy? Recall it again if necessary to describe those sensations.

2. Is the recalled joy as intense as the original joy, somewhat less intense, very much less intense, or do you only remember that you felt joy?

3. How long ago did the incident occur?"

In the case of the next *O* the order of recall was: anger, joy, fear; for the next, fear, joy, anger; for the next, fear, anger, joy; for the next, joy, anger, fear; for the next, joy, fear, anger. This variation of order was repeated with succeeding *O*s, to eliminate the influence of fatigue.

Results. Our data, setting aside for the present the observations on bodily manifestations, relate to three variables: the intensity of the revived emotion, the speed of the revival, and remoteness of the original occurrence of the emotion. As an indication of the intensity of the revival, we had the statements of the *O*s, using four grades, ranging between "as intense as the original emotion" to merely remembering that one had felt the emotion. As a measure of speed of revival, we had the stop-watch record of the time between giving the instruction to recall and *O*'s signal that the recall was complete. Obviously there was no way of insuring that different *O*s would give the signal at the same stage of completeness of the recall-process. We therefore made no statistical use of the actual times, but as will be seen made inferences only from the relative speed with which a given *O* recalled the three emotions. Similarly, in considering the third variable, the length of time since the original emotion was experienced, we took account not of the absolute length of the interval, *e.g.*, two months, a week, five years,

but merely of the relative length, noting which of the three emotions, in the case of a given *O*, dated from the most remote period and which from the most recent.

It is evident that our three variables are interrelated in such a way as to complicate the statement of results. For example, we cannot simply average the times of recall for each of the three emotions (even if we could rely on the accuracy of these times), and state that the emotion having the smallest average time of recall is revived more readily than the others; for the original occurrences of this emotion might have tended to be of more recent date, and we should expect this fact to have an effect on the speed of revival. Similarly, if we simply counted the number of cases in which a given emotion was recalled more intensely than the other three, and took these sums as indicating the relative intensity with which these emotions can be revived, we should again be neglecting the important factor of the recency of the original experience.

It is essential to begin with, therefore, that we should consider the relative recency of the original occurrence of the emotions.

In the case of anger, the original occurrence was the most remote of the three in 25 instances; it occupied a middle position in 27 instances; and was the most recent of the three in 38 instances.

In the case of fear, the original occurrence was the most remote of the three in 54 instances; it occupied a middle position in 20 instances; and was the most recent in 16.

In the case of joy, the original occurrence was the most remote in 14 instances; occupied a middle position in 47 instances; and was the most recent in 29.

It may be assumed that the remoteness of time from which an emotional experience is recalled depends upon, first, the relative frequency with which this type of emotion occurs; and secondly, the persistence of its memory. Our results show that fear dates from further back than either anger or joy. This may be explained both by the comparative rarity of the experience of fear, and by its greater frequency in childhood. Anger tends to be of more recent date than joy; whether because it is more frequent, or because the memory of earlier angers falls off more readily than that of earlier joys, does not appear.

These figures regarding the time-position of the original occurrence of the emotion may now form the basis of our other data.

Of the cases of anger recalled from most recent time, 55% were very intense revivals; of the cases of fear recalled from most recent time, 50% were very intense revivals; of the cases of joy recalled from most recent time, 62% were very intense revivals. The average of these three percentages is 55.6%.

Of the cases of anger recalled from middle position, 34% were very intense revivals; of the cases of fear recalled from middle position, 28% were very intense revivals; of the cases of joy recalled from middle position, 61% were very intense revivals. The average of these three percentages is 41%.

Of the cases of anger recalled from the most remote position, 16% were very intense revivals; of the cases of fear recalled from most remote position, 30% were very intense revivals; of the cases of joy recalled from most remote position, 45% were very intense revivals. The average of these three percentages is 30%.

It thus appears that (1), as might be expected, *revivals are less intense the more remote the time position of the original emotion.* This falling-off is not at all rapid, however, as indicated by the series: 55.6, 41, 30. (2) The falling-off is decidedly more marked for anger than for either of the other two (series: 55, 34, 16). *Revived anger falls away in intensity with time more rapidly than either joy or fear.* Joy falls away least rapidly.

(3) *Joy is more intensely revived than either fear or anger.* The sum of the percentages of most intense revivals is for joy, 168; for fear, 108; for anger, 104.

Passing to a consideration of the speed with which the revivals were made, we have the following data.

Of the cases where anger was recalled from the most recent time position, 26% had the longest time of recall of the three emotions, 22% the shortest.

Of the cases where fear was recalled from the most recent time-position, 37% had the longest time of recall and 31% the shortest.

Of the cases where joy was recalled from the most recent time-position, 36% had the longest time of recall and 37% the shortest.

Of the cases where anger was recalled from the middle time-position, 59% had the longest time of recall and 11% the shortest.

Of the cases where fear was recalled from the middle time position, 28% had the longest time of recall and 21% the shortest.

Of the cases where joy was recalled from the middle time-position, 10% had the longest time of recall and 55% the shortest.

Of the cases where anger was recalled from the most remote time-position, 41% had the longest time of recall and 25% the shortest.

Of the cases where fear was recalled from the most remote time- position, 42% had the longest time of recall and 29% the shortest.

Of the cases where joy was recalled from the most remote time-position, 24% had the longest time of recall and 63% the shortest.

From these figures the following conclusions may be drawn. (1) There *is no relation between speed of recall of an emotion and the length of time that has elapsed since the original occurrence of the emotion.* That is to say, the time of recall does not increase as the distance of the original experience increases. In the case of joy, the reverse relation almost seems to be true.

(2) *Joy is more quickly recalled than either fear or anger.* The sum of the percentages of quickest recalls is for joy, 155; for fear, 81, for anger 58. The sum of the percentages of slowest recalls is for joy, 70; for fear, 107; for anger, 126. It thus would appear that *anger is most slowly recalled.*

Finally, what is the relation between the speed with which an emotion is revived and the intensity with which it is revived?

There were in all 35 cases where anger was recalled in the most intense degree. Of these 28% were also the emotions most quickly recalled, and 28% the emotions most slowly recalled.

There were in all 29 cases where fear was recalled in the most intense degree. Of these 32% were the emotions most quickly recalled, and 43% the emotions most slowly recalled.

There were in all 53 cases where joy was revived in the most intense degree. Of these 53% were the emotions most quickly recalled, and 21% the emotions most slowly recalled.

Hence it appears (1) that *in the case of anger there is no relation between the speed with which the emotion is revived and its intensity when revived;* (2) that *in the case of fear there is perhaps some tendency for the more intense revivals to take longer;* (3) that *in the case of joy there is rather a marked tendency for the more intense revivals to be made quickly.* These conclusions would be in harmony with the often verified fact that *unpleasant experiences are recalled more slowly than pleasant experiences.*

Bodily Manifestations. Under this head, it will be recalled, we have the observations made by the *E* in the appearance and behavior of the *O* and the introspective reports of the *O* on her organic and kinaesthetic sensations. Obviously not much reliance can be placed on either of these sources of information. The chief source of error in the case of both is

that it is impossible clearly to separate those bodily manifestations that are due to the effort of attention in recalling the emotion and those that are due to the emotion itself. Although the observations of the *E* and the introspections of the *Os* have been carefully classified, they seem to justify no general conclusion regarding the special bodily reactions belonging to different emotions. For example, 'general muscular contraction' was reported introspectively in the case of joy by 17 *Os*; 'general bodily relaxation' by 26. General muscular contraction was reported in the case of fear by 36 *Os* and in that of anger by 43 *Os*; no one reported relaxation in the case of fear or anger. 'Quickened breathing' was reported with about equal frequency for all three emotions. 'Quickened heart beat' was reported most frequently (28 times) for joy; 10 times for fear, and only twice for anger: revived anger would seem to lack this characteristic.

Perhaps the most trustworthy datum of this part of our investigation concerns the *number* of bodily manifestations reported in the case of the three emotions. Since the processes of attention and effort to recall are probably not essentially different for the different emotions, any difference in the number of bodily manifestations reported is likely to be due to some character of the emotions themselves. The number of instances where a bodily manifestation was introspectively reported by an *O* was for joy, 332; for anger, 314; for fear, 282. The number of instances where a bodily manifestation was observed by the *E* was for joy, 269; for anger, 243; for fear, 220. *More bodily manifestations thus appear in the case of joy.* It will be remembered that joy was more intensely recalled than either of the other emotions. The intensity of recall for fear and anger was about equal; it is *probable*, then, that the greater number of bodily manifestations reported and observed for anger as compared with fear is due to there really being a *greater amount of observable bodily reaction in anger than in fear.*

Finally, when the results involving each of the six different time-orders of recall were compared, allowance being made for the factor of the recency of the original experience, no effects of fatigue appeared. That is, an emotion was not revived any more or less intensely or quickly for being the last or the first of the three emotions in the series as presented by the *E*.

XLVII. The Correlation of a Test of Control of Visual Imagery with Estimated Geometrical Ability

By Margaret F. Washburn, Elsie Hatt and Elizabeth B. Holt

In this Journal, vol. 25, pp. 293-295, there was described a test devised by the senior author of the present Study, of the power to control visual imagery. We have recently given this test to two groups of Vassar College freshmen, and obtained rank-difference correlations with their instructor's estimates of their ability in solid geometry. It would seem probable that such ability might involve as one of its factors the power to control, to move about, as it were, mental images of geometrical forms.

As modified for a group test, the procedure of the test for control of visual imagery was as follows. The experiment was performed at the beginning of an ordinary class hour, in the class room. A mimeographed sheet of problems was laid face downward before each student. The following directions were read aloud: "You see on the blackboard a square divided into sixteen small squares, which are numbered 1 to 16. In the upper left-hand corner square, whose number would be 1, you see a triangle.

"You will be required to imagine that you have on paper before you a square like this, divided into sixteen small squares, and you are to imagine the triangle moved about from one to another of the small squares, accord-

ing to directions which have been given you. When you have imagined the triangle moved about according to the directions, you are to write down on your paper of directions the number of the square in which the triangle would be at the end of the series of movements. For each new problem the triangle starts out on the upper left-hand corner.

"For example. Suppose your directions read: "2 to right,"—you would imagine the triangle moved to this square (pointing); "1 obliquely down to the left,"—you would imagine it moved to this square; "2 down,"—you would imagine it moved to this square; "1 obliquely up to left,"—you would imagine it moved to this square; "3 to right,"—you would imagine it moved to this square; "2 up,"—you would imagine it moved to this square; "2 obliquely down to left,"—you would imagine it moved to this square; "1 up,"—you would imagine it moved to this square; "1 to left,"—you would imagine it moved to this square; "2 down,"—you would imagine it moved to this square. This being the end of the series of movements directed, you would glance at the large square and write after the directions the number of the square on which the triangle would now be, namely, 13.

"Do not use your pen or pencil at all, not even in gesture, while you are imagining the movements. There are ten problems. Do not look at the square on the board until each problem is completed and you need to know the number of the square in which the triangle is imagined to be. Finish each problem before going on to the next. Write your name on the back of your paper."

After a pause: "When I say, 'Ready!' turn over your papers and begin. Work as rapidly as you can. As soon as you have finished the whole paper, look at the clock and write down the time. On this clock the disk shows the minutes and the hand the seconds. When you have finished, raise your hand and we will take your paper. Have you each a pencil or pen?"

The mimeographed sheet given each student ran as follows:

1. 1 obliquely down to right, 2 to right, 2 down, 1 to left, 3 up, 1 obliquely down to left, 2 down, 2 obliquely up to right, 1 to right.

2. 2 down, 2 obliquely up to right, 3 down, 1 to right, 3 obliquely up to left, 2 obliquely down to right, 1 to left, 1 obliquely up to right, 2 to left, 1 down.

3. 1 down, 2 to right, 1 obliquely down to left, 1 down, 2 obliquely up to right, 1 obliquely up to left, 2 down, 2 to left, 1 obliquely down to right, 2 obliquely up to right.

4. 1 to right, 1 obliquely down to left. 2 down, 2 obliquely up to right, 1 to right, 1 up, 1 obliquely down to left, 1 down, 2 to right, 2 up.

5. 1 obliquely down to right, 2 to right, 1 obliquely down to left, 2 obliquely up to left, 2 down, 1 obliquely down to right, 2 obliquely up to right, 2 to left, 2 obliquely down to right, 2 to left.

6. 2 down, 1 obliquely up to right, 2 to right, 1 up, 2 to left, 1 obliquely down to left, 2 down, 2 to right, 1 obliquely up to right, 2 up.

7. 3 to right, 2 obliquely down to left, 1 to right, 1 down, 1 obliquely up to right, 3 to left, 2 obliquely up to right, 1 to left, 1 to left, 2 down.

8. 2 to right, 1 obliquely down to right, 2 obliquely down to left, 2 up, 1 to left, 2 obliquely down to right, 1 to right, 2 to left, 1 obliquely up to right, 2 up.

9. 2 to right, 1 obliquely down to left, 1 obliquely down to left, 1 down, 2 to right, 1 up, 1 to right, 2 obliquely up to left, 1 to left, 1 down.

10. 2 obliquely down to right, 1 to right, 2 up, 1 obliquely down to left, 2 to left, 2 down, 2 obliquely up to right, 1 to left, 2 down, 1 to left.

Results. The scores made were as follows.

Section I (27 persons). Average time, 14 min. 48 sec.; longest time, 20 min. 20 sec.; shortest time, 8 min. 3 sec. Average number of problems correctly solved, 6.7. Highest number, 10; lowest number, 4.

Average index of efficiency (number of problems correctly solved, divided by total time in sec.), .0079; highest index, .0160; lowest, .0024.

Section II (21 persons). Average time, 13 min. 54 sec.; longest time, 18 min. 15 sec.; shortest time, 8 min. 45 sec. Average number of problems correctly solved, 6.7. Highest number, 10; lowest number, 1. Average index of efficiency, .0087; highest index, .0164; lowest, .0009.

The instructor, Assistant Professor Gertrude Smith, kindly undertook to rank the students in each section according to her judgment of their natural ability for geometrical thinking. We wish to express our gratitude for her co-operation in this and other ways.

The rank-difference coefficients found were as follows.

Section I. Speed and teacher's ranking, +.46, PE .1087; accuracy and teacher's ranking, +.16, PE .1348; speed and accuracy, +.23, PE .1306; efficiency and teacher's ranking, +.50, PE .1037.

Section II. Speed and teacher's ranking, +.36, PE .1362; accuracy and teacher's ranking, +.60, PE .1002; speed and accuracy, +.68, PE .0811; efficiency and teacher's ranking, +.53, PE .1122.

It will be seen that *good correlation is obtained in both sections between efficiency in this test and the teacher's estimate of natural ability in geometry.*

The two sections differed in the relation shown between speed of performance and accuracy. In Section I there were five persons who were very quick but inaccurate. In Section II the quick workers were also accurate. Where, as in Section I, speed and accuracy did not correlate, the instructor's estimate followed speed rather than accuracy.

XLVIII. Affective Sensitiveness in Poets and in Scientific Students

By M. F. Washburn, Elsie Hatt and Elizabeth B. Holt

In this Journal, vol. 24, pp. 583-585, the senior author of the present Study suggested as a measure of affective sensitiveness, thati s, the tendency to be strongly affected both in the direction of pleasantness and in that of unpleasantness, the ratio of the sum of the number of judgments of extreme pleasantness and extreme unpleasantness to the number of judgments of indifference, when the method was used of having an *O* express her judgment of pleasantness or unpleasantness by using the numbers 1 to 7, 1 meaning extreme unpleasantness, 7 extreme pleasantness, and 4 indifference.

The object of the present Study was to compare affective sensitiveness in two groups of *O*s, which we shall call the group of poets and the group of scientists. The poets were undergraduate students recommended by the English Department of Vassar as having distinct poetic gift. There were 33 of them: needless to say, not all budding Shelleys, but recognized by their instructors as having some measure of real gift for poetic expression. The scientists were merely students who were majoring in science, and who therefore might be supposed to have a natural bent in that direction. There were 34 of them.

Two kinds of material were presented for affective judgment: 50 nonsense-syllables, consisting of an initial vowel and a final consonant; and 50 color-combinations, two pieces of Bradley colored paper 3 cm. square pasted directly next to each other on a white card 8 by 3.5 cm. It was attempted to include in the series of syllables and of color-combinations both agreeable and disagreeable material. For the syllables, we were guided in choice by the results of the Study on The Affective Value of Articulate Sounds (this Journal, 23, 579-584). The series of syllables was first presented to an *O*, who was instructed to attend to the sound of each and judge its pleasantness or unpleasantness, using the numbers 1 to 7.

The syllables were always given in the same order, and the *E* kept the pitch and loudness of her voice as constant as possible. When the syllables had all been presented, the color-combinations were shown and judged in the same manner.

Results. The total number of 1 judgments, 7 judgments, and 4 judgments was counted for each of the two groups. The results were as follows.

Sounds. Scientists. 1 judgments, 139; 7 judgments, 28; 4 judgments, 364; index of affective sensitiveness for the group (139+28, divided by 364), .45.

Poets. 1 judgments, 189; 7 judgments, 60; 4 judgments, 361; index of affective sensitiveness, .68.

Colors. Scientists. 1 judgments, 237; 7 judgments, 88; 4 judgments, 262; index of affective sensitiveness, 1.24.

Poets. 1 judgments, 278; 7 judgments, 133; 4 judgments, 182; index of affective sensitiveness, 2.25.

Both for articulate sounds and for color-combinations the group of poets shows distinctly higher affective sensitiveness than the group of scientific students. This result is what we should expect, and indicates that this measurement of affective sensitiveness might really be of value in investigating an individual's bent.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF YALE UNIVERSITY

Communicated by JOHN E. ANDERSON

II. THE EFFECTS OF PRACTICE WITHOUT KNOWLEDGE OF RESULTS

By LLEWELLYN T. SPENCER

The distinction has been drawn between habits which are built up with constant evidence of the success or failure of a given movement, and habits whose development takes place without such guiding data. In an article published several years ago, Judd exemplified the latter type by an experimental study.[1] "The striking fact which appears in the results.... is that practice brings little if any change."[2] He found that maladaptative movements failed to be eliminated and remarked that in the course of his practice period "nothing appeared to lead to any modification or specialization of the general ability."[3]

An examination of his tables, however, leads to some uncertainty as to the real significance of his data. In computing scores when a subject was instructed to place a dot behind a screen upon what he considered the extension of a given line, Judd measured the errors as *plus* and *minus* as they were above or below the true extension of the line, and found the average error for the day and line by summing algebraically. This manifestly has an obscuring effect upon the averages especially when their numerical size is to be compared. He recognized this, and admits that the averages constitute an indication of the predominant direction rather than of the amount of error. The mean variations which he gives enable one to gauge the effect of this method of computation somewhat, but the final result is not clear and cannot be expressed numerically.

Furthermore, if his results for the first day and for the tenth (*i.e.*, last) day be averaged without regard to sign, merely as given in Table I of his paper, the average error on the first day was, in cm., 1.44, and for the tenth day, 1.34; which shows a slight improvement (7%) which appears greater when it is considered that the average error for the whole ten days was 1.30 cm., from which we may infer that the results for the last day were by no means the best. This means that somewhere along the performance-curve an improvement was scored of at least 11%.

Because of these two difficulties in supporting Judd's conclusions by his tables, the present Study was undertaken in order to discover what would be the effect of ignorance of results during practice if the errors were treated purely as errors of amount without regard to direction.

The experiment as reported by Judd was repeated as nearly as possible. Sheets of paper were prepared with lines drawn from the center of the paper to the right-hand edge at 9 different angles to the horizontal. These angles were: +60°, +45°, +30°, +15°, 0°, −15°, −30°, −45°, and −60°. Guides were fixed in the table so that a sheet bearing one of these lines might be inserted accurately and later replaced by another. A high screen was then set up at the right-hand edge of the paper so that the subject, seated before the line, could not see beyond the screen. Beyond the screen sheets were used upon which the lines had been extended previously. Upon each of these sheets the subject placed a dot where he thought the extension of the line visible to him would fall. Ten trials were made with a given

[1] *Psych. Rev. Monog. Suppl.*, 7, 1905, 185 ff.
[2] *Op. cit.*, 187.
[3] *Op. cit.*, 188.

line and then the papers were replaced with sheets drawn to a different angle. Thus each day each subject was given 10 trials with each of 9 lines, making 90 trials for each subject.

Measurements were then made of the perpendicular distance from the dots made by the subject to the extended line. These errors were then averaged without regard to sign. Three subjects, A, B and C, made the 90 trials for a practice-period of 10 days. As it was evident that in allowing the subject not only to choose his vertical location but also his horizontal location there were two variables in operation which would make the results less regular, a fourth subject, D, was used for whom the conditions were modified to eliminate the horizontal variation. A ruler 1.25 in. wide was placed beyond the screen and fitted snugly up against it. The subject was instructed to place his pencil-point against the ruler and to slide it up until he considered that it stood upon the extension of the visible line.

Table I was constructed to correspond to Table I in Judd's article. In it are shown the average errors without regard to sign with the mean variation for each line for the first day, for the tenth day, and for the 10 days as a whole. The mean variations of the averages for the whole 10 days represent the mean variations of the daily averages from the total average and not the variations of the actual errors as do the mean variations in the first two columns. They therefore give a better picture of the course of the averages through the 10 days than the crude mean variations would give. The last column of the table gives the order in which the different angles were given. The averages of the averages, given at the bottom of the columns, summarize the general tendency of the results. The data are from the results of Subject A.

TABLE I.
Subject A

	1st day		10th day		10 days		Angle of
	Av.	M.V.	Av.	M.V.	Av.	M.V.	line
1	1.60	.40	1.64	.32	2.17	.54	−60°
2	2.26	.38	1.57	.35	1.50	.45	−15°
3	1.58	.32	1.41	.29	1.58	.49	−45°
4	1.91	.41	1.30	.46	1.13	.48	+15°
5	.78	.58	.41	.19	.74	.37	+60°
6	1.63	.54	.96	.34	.98	.46	0°
7	1.16	.45	1.02	.32	.87	.25	+30°
8	1.64	.51	1.41	.29	1.40	.44	−30°
9	.54	.30	.57	.40	.51	.17	+45°
Av	1.46	.39	1.14	.36	1.21	.26	

All quantities are in cm.
Errors are added without regard to direction, *i.e.*, arithmetically.
Each line was tried 10 times each day.

The concern of the study was not to question the greater improvement of the informed learner, which Judd pointed out, but to bring out the facts as to whether or not the subject ignorant of his success or failure improved at all. The table as given both by Judd and in Table I of the present article does not represent the improvement or non-improvement of the subject very clearly. This question may be more obviously answered when results are given by days over the whole practice period. Table II gives the average error for the 9 lines for each subject for each of the 10 days. The mean variations show no significant result of practice and are omitted from the table in the interest of clearness.

TABLE II.

Average error each day for all subjects.
Italicized figures were obtained by adding algebraically, after Judd.
Otherwise quantities and computation as for Table I.

Day	1st	2nd	3rd	4th	5th	6th	7th	8th	9th	10th
A	1.46	2.07	1.38	1.02	1.28	.89	1.03	.97	.81	1.14
A	*1.27*	*1.49*	*1.14*	*.56*	*1.01*	*.34*	*1.01*	*.79*	*.91*	*1.12*
B	.68	.87	.62	.78	.48	.56	.69	.60	.90	.48
C	.68	.74	.49	.58	.56	1.26	1.04	1.35	1.54	1.38
D	1.56	1.57	1.54	1.52	1.36	1.48	1.46	1.16	1.15	1.24

In order that the degree to which the algebraic method of addition employed by Judd obscures the significance of the results may be clearly shown, the scores of Subject A have been treated as *plus* and *minus* quantities and their sums taken algebraically. These algebraic results are also given in Table II in *italics* under the averages obtained by the arithmetic method from the same results. It is readily seen that the algebraic method results in different values, which quite definitely alter the course of the general tendency especially during the last few days.

TABLE III.

Measures of Table II smoothed to give smoothed curves as in Plate I.
(For method of smoothing, see text).

Day	1st	2nd	3rd	4th	5th	6th	7th	8th	9th	10th
A	1.66	1.64	1.49	1.23	1.06	1.07	.96	.94	.97	1.03
A	*1.34*	*1.30*	*1.06*	*.90*	*.64*	*.79*	*.71*	*.90*	*.94*	*1.05*
B	.74	.72	.76	.63	.61	.58	.62	.73	.66	.62
C	.70	.64	.60	.54	.80	.95	1.22	1.31	1.42	1.43
D	1.56	1.55	1.54	1.47	1.45	1.43	1.37	1.26	1.18	1.21

Table III gives the values for the 4 subjects which result from smoothing the measures to give a more regular picture of the tendency present. The smoothing was obtained by using for each average the average of the crude average and the two immediately contiguous averages. Thus 149 in the Table under A, 3rd day, represents the average of the 2nd, 3rd and 4th day from Table II. The other values were obtained in the same way. The first and last values were obtained by averaging the contiguous value with twice the extreme value. The measures so obtained have been plotted in curves which are reproduced in the annexed plate. The presence of improvement in curves A, B, and D is very clearly evident. The graphic method makes very clear the difference in results due to the method of treatment. The curve which starts at 1.66 cm. represents the results of Subject A figured arithmetically. The curve starting at 1.34 cm. is that of the algebraically treated results for the same subject. While the presence of improvement is noted in the first half of the latter curve it is obscured in the last half. It is evident that the study of the improvement factor in the results can be adequately made only when the results are treated arithmetically.

The curve for Subject C is strikingly different from the other curves. The improvement evident in the others is shown through the fourth day, after which the curve reverses its direction and rises to a point denoting over twice as great an error as the initial score. If the other curves represent a true tendency in the practice-effects, there must be some exceptional reason for the type of curve produced by Subject C. Introspec-

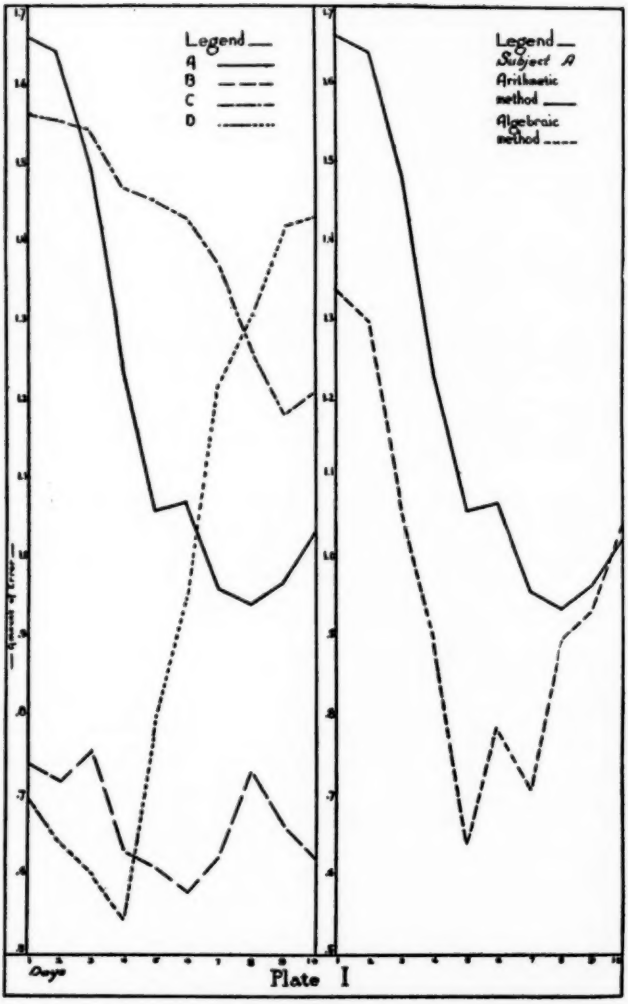
Legend
A
B
C
D
Percent of Error
Days
Legend
Subject A
Arithmetic method
Algebraic method

Plate I

tion was requested of the subject without, of course, mentioning to him the presence of anything exceptional in his performance. The subject reported that he had merely tried to estimate the projection of the line as accurately as possible during the first few days, but that he had then felt for some unknown reason that he was underestimating the distance of the line. Thereafter each estimate was carefully compensated for this supposed underestimation, with the result that the *plus* errors increased both in number and amount so that the averages soon indicated a loss rather than a gain in accuracy. If this introspective evidence may be taken as indicating the true cause of the sudden reversal in direction, the conclusion reported by Judd that maladaptative movements in practice without knowledge of results are not corrected and eliminated gains additional support.

The interesting fact is that in the other curves in general and in C's curve for the first 4 or 5 days such maladaptative movements did not occur and that improvement actually was made notwithstanding the lack of knowledge as to the success of the movements. Just how the subjects were able to improve with no indication of success or failure is beyond the province of the present Study to determine. It may be that the improvement shown represents merely the improvement of adjustment to the general conditions of the procedure, etc., and that a continuance of the experiment would show a plateau of no improvement once the situation was thoroughly familiar. This however cannot be inferred from the present results.

Summary. An approximate repetition of Judd's experiment on this question under a more direct method of treatment of the results shows that there *is* improvement in the function studied by him in three of the cases examined.

One subject attempted to adjust himself to a supposed knowledge of his results after the first few days. His reactions show a sudden change of tendency, and he became less accurate than at the start.

Conclusion. The slight improvement indicated by Judd's Table I represents an actual improvement in the habit practised which Judd's method of averaging to some extent disguised.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CLARK UNIVERSITY

Communicated by EDWIN G. BORING

XXVII. A PRELIMINARY STUDY OF THE PSYCHOLOGY OF MATHEMATICAL ADJUSTMENT

By MILES A. TINKER

The Problem

In the scientific treatment of quantitative data it is often desirable to adjust a set of observed quantities to some best fitting mathematical function. The psychologist has frequent occasion to fit distribution curves and psychometric functions to his data and the mathematical procedures for certain classes of the cases that arise are already well worked out. When elaborate procedures exist, however, there is often a tendency toward over-refinement of mathematical treatment: an elaborate and exact method is applied with great pains to data of low observational precision so that an artificial accuracy is introduced into the statement of results. When the data are secured under inconstant conditions, or when they are few in number, or when their nature is such that there is little assurance that the mathematical assumptions involved hold with rigor in the particular case, then some rougher method of adjustment is in order. The simplest form of adjustment consists in drawing 'by eye' the line or smooth curve that appears best to fit the data. In the case of the line an equation can easily be stated. In the case of a curve, although no equation can ordinarily be written, interpolations can be made, exceeding the linear interpolations in accuracy.

Our original problem was suggested by the desirability of determining the degree of accuracy with which an *O* could 'by eye' draw the best fitting line for a number of given points. Such a statement of the problem is however not explicit.

One has in the first place to define the term 'best fitting.' Adjustment by the method of least squares is usually supposed to give the 'best fitting' line, but the method of least squares is entirely arbitrary, and depends for its validity upon convention and certain mathematical considerations which render the method easy of application. Moreover, as one discovers in studying a correlation table, the meaning of this form of adjustment is equivocal. What is, one may well ask, the 'best fitting' line for the scatter of points in a correlation table: the one regression, or the other, or the line of relation lying between the two regressions and from which the regression toward the mean occurs? An *O* can draw what he regards as the 'best fitting' line for a scatter of dots, but it is impossible to test his success unless there is agreement as to what constitutes the 'best fit' or there is information as to the meaning of this term in the *O*'s mind as he sought to make the adjustment. The general problem therefore becomes two-fold: we have in the first place to determine what in a given case a scientist means by the 'best fitting' hypothesis, since it is to give meaning to data that he makes an adjustment at all; and then we can proceed to determine how well this kind of an adjustment can be made 'by eye.'

Even with the problem thus defined we can probably hope for no general solution, for the accuracy of the *O*'s performance will presumably depend upon the nature of the curve to which adjustment is to be made, and, in the case of a given function like the straight line, upon the form of

the constellation of points to be adjusted. Accuracy will presumably also vary with his degree of practice both general and specific, and with other unanalysable individual differences.

Such is the general problem. In this paper we have sought to do no more than to formulate the problem and to work out a single simple case as a type. For this purpose we selected a simple constellation of seven points for adjustment to a straight line.

An Experimental Linear Adjustment

For the experimental linear adjustment we selected the constellation of seven points given below. The points were represented by dots drawn in ink on a square of blue-lined coordinate paper 8 in. wide. The coordinates, x and y, of the points in tenths of an inch are:

$x=$	10,	20,	30,	40,	50,	60,	70;
$y=$	22,	22,	32,	42,	32,	42,	52

The *O* was asked to make an experimental adjustment by placing a transparent celluloid stencil, on which had been drawn a fine black line, over the group of dots, and moving the stencil until the lines occupied the position which in his judgment "best represented the dots." After the position had been determined to the satisfaction of the *O*, the *E* recorded the line on the paper by marking with a pencil through three holes punched in the stencil along the line.

In certain preliminary experiments we used five *O*s to make numerous adjustments for this group of points. They were given the constellation as defined by the coordinates above and also with the paper turned through 90°. In the first position the dots all lay on the heavy-faced ordinates and between the heavy faced abscissae; on turning through 90° this relationship was reversed. After making the adjustment *O* was asked to make comments upon his method of adjustment and upon any 'difficulties or doubts' that had presented themselves in making the determination.

In general we found great diversity of procedure and of result among the *O*s, so that we were forced to conclude at once that no great reliability could be placed upon observational adjustments by *O*s who were unaccustomed to the process or whose skill was unknown.

Accordingly we decided to limit our work to three *O*s only, to give them more specific directions, and to practise them in the experimental placement of the line. We selected *B* (Dr. E. G. Boring), who was accustomed casually to making such adjustments and to thinking in terms of them, *T* (the author), whose experience in these matters was progressing rapidly as work upon the problem continued, and *M* (Mr. M. K. Macdonald), who is a laboratory man with some mathematical knowledge but little statistical experience and who was entirely without knowledge of the problem, as *B* and *T* were not. These *O*s were required to make a single adjustment a day for 10 consecutive days of this same constellation of points, presented always with the same base line toward the *O*. The directions were as follows:

"Suppose the dots on the sheet of paper represent numerically observations that should theoretically fall along a straight line. You are to adjust the straight line on the celluloid stencil so that it will in your judgment have the position of the straight line which best represents these points. Make three dots along this line (through holes in celluloid) and draw a line through these points.

"Give a short description of just what you have been trying to do and how you did it."

TABLE

Average measures of deviation between the presented points, the least square adjustment, and the observed adjustment. Observers *T*, *B*, and *M*. The M.V.s show the variability of the *O* for 10 adjustments on successive days. The deviation, d, is measured in mm.

Nature of deviation	*O*	Sum of squared deviations		Sum of absolute deviations		Algebraic sum of deviations	
		Σd^2	M.V.	Σd_{abs}	M.V.	Σd_{alg}	M.V.
Points from least square line		941.57		68.71		0	
Points from observed line	*T*	948.56	6.99	69.74	.38	3.26	1.32
	B	948.68	16.13	69.46	.48	—.62	2.40
	M	1006.06	80.98	69.50	4.07	—6.58	7.54
Observed line from least square line	*T*	6.12	3.17	5.11	1.79		
	B	12.85	8.84	7.15	2.86		
	M	52.86	54.39	13.72	5.21		

The results of these 10 adjustments are given in the Table. The deviations of the presented points from the observed line were measured in mm. by dividers and the sum of the squares of these deviations, the sum of the absolute values of the deviations (without regard to sign), and the algebraic sum of the deviations (regarding the sign) were computed for each day for each *O*. The Table gives the average for the 10 trials for each of these sums, and the M.V. for the 10 trials.

The linear adjustment by the method of least squares was computed. Its equation in tenths of inches is $y = .4643\ x + 16.2851$. The Table gives the sums of the deviations of the points from this line of least squares, and also of the observed line from the line of least squares. The values of the algebraic sum of the deviations between the two lines are omitted for the reason that they do not show well the degree of approximation of the two lines.

As we have noted above the 'best fitting' line is usually supposed to be the line of least squares. In accordance with this principle the fit of a line to a scatter of points is measured by the sum of the squares of the deviations of the points from the line. The first column of the Table shows these values. The least possible sum of the squared deviations is 941.57; *T* and *B* approximate this value to the closeness of 948.56 and 948.68, whereas *M* deviates by over 60 units. The same individual differences are shown by the sums of the squared deviations between the observed line and the line of least squares: *T* is most accurate and *M* least accurate in the approximat ons of this theoretical line. *M*'s inaccuracy is probably related to his variability of performance. The column of M.V.s shows that the more variable *O* is the less accurate and that *M* was the most variable; and *M*'s protocols show that he did not have a fixed mode of adjustment.

It might be supposed now that we had answered our original problem, the problem of the degree of accuracy with which adjustment can be made observationally. A good *O* (*B* or *T*) with a fixed method and knowledge of the problem can reduce the sum of the squared deviations to within 1% of their least value (to about 948 when the least value is about 941).

Immediately, however, the problem arises whether the line of least squares is the 'best line' and whether the sums of the squares of the deviations are the best measure of goodness of fit.

If one enquires now concerning the nature of the 'best' hypothesis for the linear representation of a given set of points, one might be justified in contending that the best line is the line which most nearly fulfills the conditions which the scientist has in mind when he seeks to let a non-linear set of observed points be represented by a line. To argue to representativeness on purely mathematical grounds is to make the meaning of the line clear but not necessarily to give it the meaning which the scientist has in mind. The answer thus lies in the mind of the scientist, and we sought from the protocols of our *O*s to discover what they, as scientists, meant by a line which 'best represents' a constellation of points.

The protocols showed a similarity between *B* and *T* in their methods of adjustment. *B* initiated his adjustment empathically, balancing the dots on two sides of the line and regarding them as deviating from the line in a direction perpendicular to the line. After the completion of the initial empathic adjustment, he checked or corrected his setting by noting whether the sum of the distances of the dots above the line appeared to equal the sum of the distances of the dots below, taking all these distances now vertically to the base line and therefore diagonally to the observed line. *T*'s method was practically identical with *B*'s; he sought to make the dots balance about the line at first and then to check the result by equating the sum of the distances above to the sum of the distances below. Both *B* and *T* sometimes accomplished this final equation by pairing off superior and inferior dots against each other. *T* held to his method more rigorously than did *B*, who sometimes sought to make the entire adjustment empathically. This difference may account for the fact that *T* is less variable than *B* by all criteria computed (see the M.V.s of the Table).

M did not in his 10 trials achieve any fixed method. He began by selecting the left hand dot as a pivot, passing the line through it and then swinging it until a balance of dots on either side had been obtained. He secured this balance by summing up the vertical distances above and below the line. Later he abandoned the pivot and sought to obtain the balance with the line free within the field, but in the very last trials he returned again to the use of the pivotal dot. This change in procedure seems to account for the fact that he is the most variable *O* (see Table).

It will be seen that all three *O*s begin by obtaining a balance or empathic adjustment and that this initial adjustment seems to them approximate only. They then proceed to a finer adjustment, and quite independently of one another seek to equate the sum of the vertical distances above the line to the sum of the vertical distances below. Their selection of the vertical distances instead of the horizontal or the perpendicular deviations may be due to the fact that the dots to be adjusted lay on the heavy-faced ordinates of the cross-section paper.

No *O*, it is to be noted, in making the adjustments sought to think in terms of squared deviations, although all three were accustomed to use the method of least squares and two were aware that the usual form of adjustment would require them so to think.

If now, following the hint contained in the protocols, one seeks to substitute for the line of least squared distances some other line which is determined without regard to the squares of the deviations, one then discovers quickly enough the reason for the popularity of the hypothesis of least squares. What might this other line be? It might be the line for which the sum of the unsquared deviations is least. But this line is indeterminate, for the least sum is zero when the positive and negative deviations are equal and any line of constant slope moved through the constellation passes through a position where this sum is zero. It is for this reason that the text-books tell us that the method of least squares is

of advantage because in squaring the deviations it makes them all of like sign. But can we not simply arbitrarily dispense with the signs and sum up the deviations in their absolute values? Such a line is also not necessarily determinate; when the number of points is even and half lie on either side of the line, then all positions fulfilling this condition give for a constant slope the same sum of the absolute deviations. It is possible, however, to combine the two criteria. By applying simultaneously the condition that the algebraic sum of the deviations shall be equal to zero and the sum of the absolute deviations a minimum, and then effecting the solution by assuming enough values of the slope to indicate the slope that will give the required minimum (interpolating parabolically to find the minimum), we obtained, as a line satisfying these two conditions: $y = .4631\ x + 16.3331$. This line differs so little from the line obtained by the method of least squares that we deemed separate mathematical treatment unnecessary, but it is interesting to note that it can be obtained in cases where it is desired.

The foregoing discussion makes it evident that the *O*s did not tell the whole tale in their protocols. To equate the sum of the positive to the sum of the negative deviations is not to determine the line: only if the slope be given or a single pivot be fixed upon will this process determine the line. It would seem that *B* and *T* tended rather to fix the slope on general empathic grounds and to shift the line up or down in equating positive and negative deviations, whereas *M*, at least when he was using the 'pivot,' probably selected the pivot empathically and then adjusted the summed deviations by rotation.

Although the protocols are not fully descriptive of the process, there is no doubt that the *O*s failed to attempt to apply a method of least squares in their observational adjustments, although they used in part a method of the equation of the sums of the positive and negative deviations. It was of interest therefore to discover whether some measure of deviation more in accord with the *O*'s avowed intent in adjustment would give results different from those from the sums of the squared deviations. We have therefore presented in the Table as measures of deviation the sums of the absolute deviations and the algebraic sums of the deviations for the three *O*s for the case of the deviations of the points from the observed line.

The Table shows that, although individual differences in variability are shown to be the same by any criterion, the degrees of approximation of the dots to the observed line vary considerably with the criterion. If we take the sum of the absolute deviations as a measure, then we find that all *O*s do about equally well, and that if small differences are taken into account the rank order of accuracy is *B*, *M*, *T*; whereas when squared deviations are considered *M* deviates widely from the other two *O*s and the rank order of accuracy is *T*, *B*, *M*. If we take the *O*s' own criterion as given in their protocols, *viz.*, that the algebraic sum of the deviations should be zero, we find that the line of least squares fulfills the conditions perfectly (necessarily), and that there are individual differences this time in the rank order *B*, *T*, *M*.

In the face of such difference it is impossible to contend that the measure of deviation makes no difference in the conclusions. The problem seems to call for a further analysis, logical and perhaps introspective, of the meaning and scientific use of adjustment, with an adaptation both of the form of adjustment and of the measure of its degree to the scientific problem involved, rather than a submission to principles of mathematical simplicity which have little correspondence with the psychology of scientific thinking.

Conclusions

Under favorable conditions of practice and constancy of attitude, observers can make a linear adjustment of a small number of points with

an inaccuracy that is only about 1% greater than the least inaccuracy that can occur as measured by the method of least squares.

Observers do not, however, work by least squares, nor think in terms of squared distances. The psychology of adjustment is still obscure in that it is in part empathic, but it includes as an important factor the equation of the sums of positive and negative unsquared deviations. It is therefore not apparent that the method of least squares has as much scientific meaning as might some other hypothesis.

The applications of different criteria of the goodness of fit of an adjustment give different results. It appears therefore that goodness of fit depends upon the scientific meaning to be attached to the deviations of the points from their adjustment, and that no 'best' line can be defined until a criterion of deviation from it is thus established.

XXVIII. Attention as a Condition of Affection

By Max Meenes

In the psychological laboratory it is ordinarily assumed that attention is a condition of affection: if the affective value of a stimulus is to be determined the stimulus must be attended to.[1] On the other hand the psychoanalysts assert that the affective tone of consciousness may be determined by ideas obscurely or unconsciously present. There would, of course, be no excuse for confusing *a priori* the "unconscious" of the psychoanalyst with the "unclear" of the experimentalist; but there exists nevertheless an obvious problem for the experimentalist in the determination of the affective value of the stimulus which is unattended to. Yokoyama[2] recently touched upon this problem in his study of the simultaneous effect of color and form upon attention, but he was unable to solve it because his series did not include a complete simultaneous variation of the two variables. The present study, suggested by Yokoyama's problem, seeks to compare, by the method of paired comparisons, the affective values of colors attended to with the affective values of the same colors when attended from.

Stimuli. We used six Milton-Bradley papers: "red" (R), "orange" (O), "yellow" (Y), "green" (G), "blue" (B), and "red violet" (P). Every stimulus consisted of a pair of these colors, arranged in immediate left-right juxtaposition, with each member of the pair a square 5 cm. on the side. In making a comparison one such paired stimulus was presented above to be compared with a second paired stimulus presented below. The distance between these upper and lower paired stimuli was 8 cm. *O* was 1.5 m. from the stimuli, which were observed under the artificial daylight of the C2-Mazda lamp. The stimuli were presented through windows in a gray cardboard screen and exposed for 2 sec.[3]

The series included every possible pair of the six colors including every color paired with itself. There are 15 heterogeneous pairs and 6 homogene-

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[1] The relationship is more often implicit in the experiments by the method of impression than explicit in systematic exposition; nevertheless *cf.* E. B. Titchener, *Beginner's Psychology*, 1915, 79f.

[2] Yokoyama, M., Affective tendency as conditioned by color and form, this Journal, 1921, 32, 81-107, esp. 97-100.

[3] The apparatus and conditions were similar to Yokoyama's: *op. cit.*, 83ff.

ous pairs (color with itself), making 21 paired combinations in all. To have reversed every pair in order to give both space orders would have ultimately required 10,332 judgments from each *O* to complete the experiment. It would not have been possible to obtain so many judgments in the time available; hence we reduced the labor by presenting the pairs without reversal, — a reduction from 10,332 to 1260 judgments, for the work increases approximately with the square of the number of stimuli. We took account of space error, however, by making up the pairs in such a manner that every color came as often at the left as at the right. The list of stimuli prepared in this manner is as follows:

1. RR	8. YO	15. GP
2. OO	9. GY	16. GR
3. YY	10. BG	17. BO
4. GG	11. PB	18. PY
5. BB	12. RY	19. RB
6. PP	13. OG	20. OP
7. OR	14. YB	21. PR

Procedure. Every one of these 21 stimuli was presented with every other one by the usual method of paired comparisons in both vertical space orders, *i.e.*, although the right-left space order within the paired stimuli was taken account of only indirectly in the manner described, the up-down space order was taken strict account of by actual reversal under observation. This comparison required 420 judgments.

Such a series of 420 judgments was taken (1) with attention upon the right members of each paired stimulus, (2) with attention upon the left members of each paired stimulus, and (3) with attention equally upon both members of each stimulus, thus making the entire experiment consist of 1260 judgments for each *O*.

The instructions were as follows: "After the preparatory signal you will be shown two pairs of colors, one pair above the other. Sometimes you will be instructed to pay attention only to the right color of each pair, sometimes only to the left color of each pair, and sometimes to both colors in each pair. Give yourself up to the colors that you are told to attend to and report as quickly as possible which of the stimuli attended to is the more pleasant, whether the 'top' or 'bottom'. Do not form any associations with these colors if you can help it; simply live them and state immediately the more pleasant. Whenever you fail to live up to these instructions, report 'failure'. If the stimuli attended to are equally pleasant report 'same'."

The series were balanced against temporal effects by requiring successively 210 judgments each for (1) attention to the right, (2) attention to both, (3) attention to left, (4) attention to left, (5) attention to both, and (6) attention to right.

"Failures," which were not often reported after practice, were repeated when reported.

Observers. The *O*s were Dr. Marjory Bates (*Ba*), then a graduate student in psychology; Dr. M. Yokoyama (*Y*), assistant in psychology; and Mr. F. L. Bixby (*Bi*), an advanced undergraduate.

TABLE I

Observer *Ba*

A. Preferences for paired colors where one color of the pair is attended to and one attended from. Method of paired comparisons; maximal number of preferences 40. Colors attended to in caps (B, R, G, *etc.*); colors attended from in lower case (y, r, g, *etc.*). Each cell gives the preferential score for a given color attended to (column) when combined with a given color attended from (row). Columns and rows are arranged so that average scores are in rank order.

Colors attended from	COLORS ATTENDED TO							
	B	R	G	Y	O	P	Av.	m.v.
y	39	36.5	24.5	10.5	13.5	20	24	9.3
r	35.5	20	19.5	24	17	9.5	20.92	5.9
g	36.5	30.5	17	16.5	18	4	20.42	8.7
b	32	32	18.5	17	17	5.5	20.33	5.5
o	37.5	33.5	23	10.5	10.5	6	20.17	11.1
p	30.5	16.5	19.5	24	8.5	8	17.83	6.8
Av.	35.17	28.17	20.33	17.08	14.08	8.83		
m.v.	2.6	6.7	2.3	4.6	3.2	4.0		

m.v. of the Av.s = 1.23
Av. of the m.v. s = 8.3

m.v. of the Av.s = 7.37
Av. of the m.v.s = 3.9

B. Preferences for paired colors where both colors of the pair are equally attended to. Method of paired comparisons; maximal number of preferences 40 (doubled in order to be comparable with part A). Each cell gives the preferential score for the pair indicated by the column and row in which it lies. Columns and rows are arranged so that average scores are in rank order.

Colors	COLORS							
	Y	B	R	O	G	P	Av.	m.v.
Y	18	34	37	17	25	30	26.83	7
B	34	34	23	27	27	12	26.16	5.9
R	37	23	23	27	13	1	20.67	9.1
O	17	27	27	11	21	8	18.5	6.5
G	25	27	13	21	15	9	18.33	6
P	30	12	1	8	9	8	11.33	6.4

m. v. of the Av.s = 4.09
Av. of the m. v.s = 6.8

TABLE II

Observer *Y*

See legends of Table I

A. One color of pair attended to and the other attended from.

Colors attended from	COLORS ATTENDED TO							
	G	O	B	Y	R	P	Av.	m.v.
y	30	24.5	29.5	26	13	12.5	22.58	6.6
g	36	25.5	23	19.5	12	9	20.83	7.3
o	33	30	28	15.5	10.5	5	20.33	10.
b	19	28	28	26	4.5	8	18.92	10.7
p	17.5	19.5	17.5	22	9	9	15.75	6.3
r	25	22	17	18	12	6	16.75	5.2
Av.	26.75	24.92	23.92	21.17	10.18	8.25		
m.v.	5	3.4	4.7	3.5	2.3	2		

m. v. of the Av.s = 2.07
Av. of the m.v.s = 7.7

m. v. of the Av.s = 6.65
Av.s of the m.v.s = 3.5

B. Both colors of pair attended to.

Colors	COLORS							
	G	Y	B	O	R	P	Av.	m.v.
G	37.5	35.5	29	32.5	20	14	28.08	7.4
Y	35.5	24.5	35	23	12	14.5	24.08	7.6
B	29	35	27	30.5	13.5	6.5	23.58	9.2
O	32.5	23	30.5	28.5	3.5	8.5	21.08	10
R	20	12	13.5	3.5	16.5	1.5	11.17	5.9
P	14	14.5	6.5	8.5	1.5	6.5	8.58	6.5

m.v. of the Av.s = 6.38
Av. of the m.v.s = 7.8

TABLE III

Observer *Bi*

See legends of Table I

A. One color of pair attended to and the other attended from.

Colors attended from	COLORS ATTENDED TO							
	R	B	G	P	O	Y	Av.	m.v.
y	34.5	34	26.5	18	13	1.5	21.25	10.4
r	32	31.5	21.5	17.5	16	3.5	20.33	8
g	32.5	32.5	21	17.5	12.5	4	20	8.7
p	31.5	32	22	17.5	13.5	3.5	20	8.5
b	36.5	32	19	15	14	2	19.75	10.4
o	31.5	33	22.5	19	10	2	19.67	9.3
Av.	33.08	32.5	22.08	17.41	13.16	2.75		
m.v.	1.6	1	1.6	0.8	1.3	0.9		

m.v. of the Av.s = 0.43
Av. of the m.v.s = 9.7

m.v. of the Av.s = 8.89
Av. of the m.v.s = 1.1

B. Both colors of pair attended to.

Colors	COLORS R	B	G	P	O	Y	Av.	m.v.
R	28	37.5	34.5	25	25.5	24	29.08	4.6
B	37.5	26	32.5	25.5	22	22.5	27.67	5
G	34.5	32.5	13.5	18	22	14	22.41	5
P	25	25.5	18	15	13.5	9	17.67	5.2
O	25.5	22	22	13.5	8.5	3.5	15.83	7.3
Y	24	22.5	14	9	3.5	0	12.17	8.0

m.v. of the Av.s = 5.58
Av. of the m.v.s = 5.2

TABLE IV

M.v.s of the averages of preferential scores in their absolute amounts (see Tables I-III) and as percentages of the dispersion of a maximally determined series. The m.v. of a series under the method of paired comparisons where preferential determination was invariable would be 10.5.

		ONLY ONE COLOR OF PAIR ATTENDED TO		BOTH COLORS OF PAIR ATTENDED TO
	Obs.	For color attended to	For color attended from	
	Ba	7.37	1.23	4.09
M.v.	*Y*	6.65	2.07	6.38
	Bi	8.89	0.43	5.58
M.v. as	*Ba*	.70	.12	.39
% of	*Y*	.63	.20	.61
10.5	*Bi*	.85	.04	.53

Results. The results are tabulated in Tables I-III.

Part A of each of these tables gives the number of preferences for each pair of colors. The column indicates the color of the pair attended to and the row the color attended from. Thus the rows give the preferential functions for the colors attended to when the color attended from is constant; and the columns give the preferential functions for the colors attended from when the color attended to is constant. The highest possible score would be 40, since each paired stimulus was presented 40 times with attention on one member of the pair and 40 times with attention on the other member. Neither space-error (right-left, up-down) appears in the table, since all the results have been combined.

Part B of each table gives similar results for the series when both colors of the pair were equally attended to. Actually the table should be only a half table, but it has been filled in completely and the scores have been doubled in order to make the scores comparable in magnitude and the table comparable in form to Part A.

It is apparent from the tables that the colors attended to are the predominating factors in the determination of the preferences. The evidence for this conclusion is as follows:

(1) The preferential orders of the colors are much more widely dispersed for the colors attended to than for the colors attended from. The m.v.s of Tables I-III are brought together in Table IV to show this relationship. If we were dealing with a univocal determiner in either set of colors the score for the most pleasant pair would be 40: the score for the

next most pleasant pair would be 38, since this pair would be judged more pleasant than any other pair except twice when compared with the most pleasant pair; and so on. The m.v. of such an absolutely determined series would be 10.5, and the greater the m.v. of one of these series the greater would be the dependence of the preferences which it expresses upon the differences which the series represents. Table IV shows that the dispersion for the color attended to is from 3 to 20 times greater than for the color attended from. The color attended to shows 63 to 85% of the maximal dispersion possible; the color attended from only 4 to 20%.

(2) The same relationship is shown in a different manner by the averages of the m.v.s in part A of Tables I-III. The av. m.v. at the right of the table is the average dispersion when the color attended to is the variable, and it is consistently larger than the av. m.v. at the bottom of the table which is the average dispersion when the color attended from is the only variable. In the one case the dispersions are from 2 to 9 times the dispersions in the other.

(3) It would seem that a further test of the relative importance of the color attended to and the color attended from might be made by comparing the preferential function for each with the preferential function for the colors when both are attended to (Part B of the tables). The method is not satisfactory on statistical grounds, however, for the reason that the series involved is in some cases of low precision, as indicated by the m.v.s of Table IV. One gets high coefficients of correlation (.70 to .98) between the functions for the colors attended to and the functions where both colors were attended to, but one may also by chance get high correlation between the functions for the colors attended from and the functions for both colors since the former are all of low precision.

The further question arises whether the colors attended from, although they have no great effect upon the preferential judgments of the paired stimuli, have any demonstrably consistent effect at all. It is not to be expected that the preferential functions of these colors (columns of Part A, Tables I-III) should be indeterminate. Even if the colors had no effect in themselves, chance differences would presumably appear in the columns, making rank orders statable. The problem is simply whether the rank orders of the colors attended from are significant.

One turns for the solution to a comparison of the preferential functions for the colors attended from and the corresponding functions for the colors attended to. If, given everything else constant, the colors attended from do cause the paired stimuli to fall in a preferential function of the same order as the colors attended to, then it would seem that color is still an effective, if slight, determinant under inattention.

A positive conclusion on this point can not, however, be reached from our results. For *Y* the correlation between the series of averages for the colors attended to and the colors attended from (Part A of the table) is .81; agreement is high. For *Ba* there is almost no agreement; the correlation is .18. *Bi*, the least skilled *O*, brings Y from one end of the series to the other, thus giving a negative correlation, −.53. In the face of such unaccountable variability it is tempting to ascribe the differences to "chance" and to say that the colors attended from have no demonstrable effect at all. The conservative conclusion is, however, that their effect, if it be that they have a consistent effect, is not the same from *O* to *O* nor in general of the same nature as the effect of the colors attended to.

Conclusion. In paired comparisons of colors affective judgments are conditioned predominantly upon the colors attended to. Colors attended from, but presented with the colors attended to, have at most but slight effect upon the judgments.

BOOK REVIEWS

Hugo Münsterberg, His Life and Work. By MARGARET MÜNSTERBERG. D. Appleton and Co., New York, 1922, pp. x, 449.

The shape in which Miss Münsterberg has set forth the life of her father is quite different from the usual form of biographies. From the nature of the case it could hardly have been a judicial survey of Münsterberg's life interwoven with a critical estimate of his works. In lieu of the latter the author has worked out an appendix of 147 pages, made up of abstracts of her father's writings. A classification of these under the headings psychological, philosophical, educational, biological, ethical, social, political, medical, legal, aesthetic, economic, and belletristic, gives some notion of the extent of Münsterberg's activities as well as of his unwearied industry and facility of expression.

The book is also not a biography in the form of "Life and Letters;" for, regrettably, but few of Münsterberg's letters are included in the volume which runs true to its title "Life and Works" and chiefly and especially works that appeal to the contemporary public. As much space is given to the Congress of *Gelehrte* at the St. Louis fair, in great part organized by Münsterberg, as to the first twenty-nine years of his life; and the account of the investigation of the criminal Orchard is noticed as much at length as the transactions of the Freiburg laboratory,—psychologically the most fruitful period of her father's life.

Perhaps Miss Münsterberg felt that this division of labor was justified in the case of one so much in the public eye as was her father, and further that the scanty treatment of his professional work in the first part of her book is amply compensated by the abstracts of the appendix. But such a plan mars the integrity of the work.

Assuredly there is enough material in an account of Münsterberg's exoteric activities, from the time he set foot in America, to fill a fair-sized book. With his name is associated the establishment of Emerson Hall and the Germanic Museum at Cambridge, the planning and execution of the congress of "World's Scholars" at St. Louis, and the founding of the American Institute in Berlin. In addition he treated psychopathic sufferers, investigated trance mediums, advised business houses, and lectured extensively outside of the University. He was a steady attendant upon congresses of Psychology and Philosophy and yet found time to dispense a liberal hospitality which in its far flung scope included such diverse personalities as Charles Eliot Norton, Prince Henry of Prussia, and the actress Nazimova. And all this on top of his routine of academic work, and along with a steady stream of books, essays and magazine articles! The man's industry was tremendous and his facility of expression prodigious, especially when one considers that the greater part of his publications was in a language which he practically learned after his advent in the United States.

Münsterberg's power of expressing himself copiously, fluently, and directly, whether in English or German, was of course a gift which required practice for its development; no more in writing than in piano-playing does one acquire facility without practice. For Münsterberg the practice began in his boyhood with the writing of verses and plays. Later he published a volume of poems. And the pen of a ready writer was no negligible quantity in shaping his career. It certainly was one of the factors in influencing James to recommend him to Harvard. James' aversion to the cumbrous and involved style of German systematic philosophy amounted to repulsion, and his delight in finding a German who could write psychology clearly and directly was pronounced. "You handle things with such a

broad, light touch in writing about them," he tells Münsterberg in one of the letters which are the high lights in the "Life and Works," "that it is a constant pleasure to read what you say."

What James had read was the *Willenshandlung* and two or three numbers of the *Beiträge*. Curiously enough, but little notice is given to the *Beiträge* by Miss Münsterberg, though they belong to the period of her father's life when his professional work was at its best. They were also decisive factors in bringing him to Harvard. "I must say," James wrote him, "that you seem to me to be doing more to open out vistas in Psychology than anyone today."

Of course James was no experimentalist; in fact he detested experimental work; and it was at this period of his life he had declared that the psychological outcome of Fechner's work was "just nothing." In England Robertson and Alexander also gave the *Beiträge* hearty welcome. On the other hand, the work was sharply criticised by the experimentalists Martius, Meumann and Titchener. G. E. Mueller ran through the *Beiträge* with a fine-toothed comb dipped in vitriol. In a fine letter of consolation to Münsterberg, James called this critique "brutal." Stumpf in Berlin was perplexed to find his own work confirmed by Münsterberg by methods which he (Stumpf) thought invalid. The task which Münsterberg had set himself in publishing the *Beiträge* was beyond the power of anyone to carry out; it was to perform all the experimentation himself. The outcome was a series of studies top-heavy with hypothesis. Rich in suggestion and fertile in experimental expedients, Münsterberg somehow gives the impression that experiments must need bear out his hypothesis. However, the work made enough of an impression on James to bring about a call to Harvard where the early volumes of the Harvard Psychological Studies give more evidence of Münsterberg's fertility in ideas.

Münsterberg's *magnum opus*, the *Grundzüge der Psychologie*, was dedicated to James, though the latter gave warning that the work might find in him "one of its worst enemies." This, of course, with James would imply no change in personal regard. Whether, however, the entente of the earlier days was kept up, we do not learn from the "Life and Works." James' letters, as time went on, seem to have become fewer and shorter. He could hardly have regarded with approval Münsterberg's absorption in applied psychology. In any case, contrasting if not opposing temperamental tendencies had become pronounced, Münsterberg enjoying the throngs of the market-place, while James detested crowds—psychological congresses included. Somewhere he writes of his dislike of "big standup receptions where people scream and breathe in each other's faces."

Of the scholarly *Grundzüge* only the general part, the Principles of Psychology, was published. Some fourteen years later Münsterberg brought out his "Psychology General and Applied," shaped on the principles laid down in the *Grundzüge*. The work was written as a text-book, and was hardly a worthy successor of the *Grundzüge*. As a text-book, it introduced novel sections on Social and Applied Psychology, but it was too shot through with epistemological puzzles to take the place of the common form of manual.

In these later days Münsterberg had pretty well given himself over to applied psychology, and from his pen flowed a steady stream of 'applications'. "Psychotechnik," "Psychotherapy," "Psychology and the Teacher," "Business Psychology," and so on. While all these publications resulted in making Münsterberg the best known psychologist in America, they were as a whole in no wise helpful to his reputation as a scientific psychologist or to the science of psychology. Despite much sound advice and scholarly admonition, they formed the gate for the flood of pseudo-psychologising and commercialized psychologues now inundating this country.

In the later years of his career, Münsterberg was deeply interested in bringing about an entente between Germany and the United States. To

this he sometimes added England, though in this respect his activities do not seem to have gone much beyond a pious wish. His anxiety to maintain the entente between his adopted and native countries during the Great War, and the coldness with which some of his colleagues met him in those days, contributed in all likelihood to his tragic death. That his preference was for this country his refusal of the call to Königsberg in 1905 attests: the prospect of a call to Oxford (which, *pace* Miss Münsterberg, is far from being a "call") was probably no great temptation. But that twenty-two years of residence over here had wrought no fundamental change in the Germanism of his nature is curiously shown in a eulogy to the Emperor William II., who had been selected along with Wagner, Schopenhauer and Nietzsche as representative writers for a volume of Kuno Francke's "German Classics."

Miss Münsterberg has had a very difficult task to perform, and she seems to have chosen to guard against filial fondness by making her account as *objectiv* as possible; but the absence of letters deprives the biography of that which counts most in biography—the disclosing of personality. From the "coign of vantage" of the reviewer, I would counsel her, in case of future editions, to scan her proofs more narrowly, to verify all quotations, especially from Shakespeare, and above all, to follow d'Alembert's advice to Voltaire as regards economy in the use of adjectives.

Stanford University FRANK ANGELL

Brain Abscess: Its Surgical Pathology and Operative Technic. By WELLS P. EAGLETON, M.D., Newark, N. J. (President American Otological Society, 1921.) The Macmillan Co., 1922.

As stated in the preface, this monograph is the result of extensive experience in cranial surgery. It is original in conception and aims to induce the surgeon to think in terms of pathology and to plan his operation so that it may be adapted to the pathological condition found in any particular case, and at the same time to utilize surgically the active protective forces of the brain.

Discarding the older ideas of the aural and nasal origins of the infection, the author bases his classification upon the mode of entry into the brain of the different pathological processes causing brain abscess. By utilizing this classification the surgeon is assisted in determining not only the existence of an abscess, but also the presence or absence of a capsule, which is of great importance.

Part I. deals with the general aspects of intracranial surgery, Part II. with the special consideration of the surgical pathology and operative technique of brain abscess, while Part III. is devoted to its surgical diagnosis. The text is divided into a description of the microscopical lesions of brain abscess, followed by the physiological and mechanical conditions which must be dealt with surgically, and concludes with a description of the technique best adapted to meet them. By this arrangement the reader is enabled to peruse a general treatise on the pathology and surgery of brain abscess without the tiresome details of operative technique, which follow in their proper place, in small type, at the end of each section.

Although the book is primarily a record of personal experience, over seven hundred references are given from recent literature. It also includes a compilation and analysis of the 125 post-mortem records of cerebellar abscess which had been reported up to 1919; and 141 recorded cases of frontal lobe abscess, in addition to 31 personal case reports, with the diagnostic and surgical lessons learned from each. It is an encyclopedia of information on the surgical pathology and operative treatment of brain abscess.

The illustrations, with two exceptions, are from original drawings and photographs.

Chapters I. and II. contain brief directions regarding the method of approaching the case, the proper time for operation, and describe the general surgical technique for intracranial operations. Their perusal will give valuable assistance to any surgeon who may be called upon to make an intradural exploration.

Chapters III. and IV. deal with the pathology of brain abscess, call attention to the importance, from a surgical standpoint, of the presence of a capsule, giving points as to how it may be diagnosed and utilized. Stress is also laid on the value of the protective processes of the brain and the importance of conserving them.

In chapter V. the author gives his views on metastatic abscess, which it will profit the reader to consult in the original.

Chapter VI. deals with cerebellar abscess. It aims to help the surgeon to localize the abscess from a study of the mode of entry.

In chapter VII. abscess of the frontal lobe is considered and, as stated in an introductory note, it is largely the result of a study of the cases reported in the literature. It emphasizes the importance of the protective character of the nasal and accessory sinus membranes, and the danger associated with their injury during operation.

Chapter VIII. is concerned with hernia cerebri, and Chapter IX., on the protective mechanism of the brain, explains the action of the delicate processes within the dura which must be preserved and encouraged if a case of brain abscess is to recover following an operation. It is based on the experimental work of Flexner and others, and is the first attempt to correlate this work and make it applicable to brain surgery.

In Part III., chapters X., XI. and XII., the diagnosis of brain abscess is very exhaustively treated. The symptomatology peculiar to brain abscess is divided into (1) evidence of cerebral suppuration, (2) symptoms of cerebral compression, (3) localizing symptoms. The symptoms are explained on the basis of the physiology of the nervous system by giving, as far as possible, the anatomical and physiological reasons for the different symptoms as they develop. The diagnostic value of "little things" is emphasized, *e.g.*, the "initial vague chill" in the diagnosis of a capsule; the "disproportion" of "pathological sequences" in diagnosis, and the possible cause of subnormal temperature, are considered. The literature is filled with records of cases where a diagnosis was not made "because of the absence of the, so-called, symptoms of brain abscess." Review of these cases will show that what in reality prevented the surgeon from making a diagnosis was not the absence of the symptoms of cerebral suppuration—for they were present—but the absence of those symptoms which point to an increase of the intracranial contents, the signs of cerebral compression; and few cases of brain abscess do present evidences of cerebral compression in the early stages, while in a number of cases of frontal lobe abscess compression never occurs at all. Headache, according to the author, is always present, and no other single symptom is of such uniform occurrence or of greater assistance to a positive diagnosis. Early diagnosis is of the utmost importance if operative interference is to be successful.

The presence or absence of papilloedema and the value of "transient symptoms" such as aphasia and hemianopsia for colors are discussed. The frequency and importance of this type of hemianopsia as an early diagnostic sign of adjacent temporo-sphenoidal lobe abscess have not been previously recorded.

In chapter XIII. the diagnosis of frontal lobe abscess is dealt with, and in chapter XIV. certain complications that may arise are considered.

While this book covers a restricted field, the subject is treated exhaustively, and it is not too much to say that it should be in the hands of every cranial surgeon. It will also be of the greatest service to the otologist and

rhinologist, and, because of its extensive discussion of the presence or absence of papilloedema, it will appeal to the ophthalmologist. Even to the general surgeon, who any day may be called upon to perform an intradural operation, the book will be a valuable aquisition, and because of the wealth of diagnostic details it may be recommended to the neurologist and the general practitioner. In fact, as the fruits of an extensive experience, and careful and discriminating observation, it may be heartily recommended to all medical men.

The paper and printing are excellent and the general arrangement of the material leaves nothing to be desired. Author and publisher alike are to be congratulated on the production of this important contribution to brain surgery.

Cornell University SUTHERLAND SIMPSON

Instinct and the Unconscious. By W. H. R. RIVERS, Cambridge University Press, 1920. pp. viii, 252.

This volume by the late Dr. Rivers is, its subtitle tells us, "A Contribution to a Biological Theory of the Psychoneuroses." The book has two parts. "The first gives the substance of lectures delivered in the Psychological Laboratory at Cambridge in the summer of 1919, and repeated in the spring of the present year (1920) at the Phipps Clinic of the Johns Hopkins Medical School, Baltimore, under the direction of Professor Adolf Meyer. The second part consists of appendices in which are republished occasional papers written as the result of clinical experiences gained during the war." "The general aim of the book," as the Preface goes on to state, "is to put into a biological setting the system of psychotherapy which came to be generally adopted in Great Britain in the treatment of the psycho-neuroses of war."

One may note at the outset that this system is essentially a modified Freudianism in which the instinct of sex was found to play a negligible part: the general conception of instinctive nature upon the basis of which these pathological psychoses are explained and treated being that current in British writings since McDougall's comprehensive treatment of the subject in his "Social Psychology." These views are supplemented and interpreted in their biological aspects by reference to the work of Henry Head and his collaborators in neurology. Thus the mechanisms of diseased conditions of mind are, in general, reduced to suppressed instincts. "The aim of this book," we are told, "is to consider these mechanisms in their relation to the more normal processes of the animal organism, and especially to the mechanism by which certain parts of experience become so separated from the rest that they are no longer capable of recall to consciousness by the ordinary processes of memory. Psycho-neurosis depends essentially upon the abnormal activity of processes which do not ordinarily enter into consciousness, and the special aim of this book is to consider the general biological function of the process by which experience passes into the region of the unconscious. I shall attempt to show that the main function of psychoneurosis is the solution of a conflict between opposed and incompatible principles of mental activity. Instinctive processes and tendencies, and experiences associated therewith, pass into the unconscious whenever the incompatibility passes certain limits" (pp. 5, 6).

The author supports his theory of the nature of these conflicts and their mechanisms by reference to Head's distinction of *protopathic* and *epicritic* sensibility, and the general function of the neo-pallium in providing for mass-action of the all-or-none type accompanied by a strong affective tone. The indiscriminative character of protopathic sensibility, together with the emotionally toned responses controlled by the *optic thalamus*, furnish a biological setting for the most original of the instinctive tendencies. *Sup-*

pression is defined biologically as the natural and 'unwitting' process whereby outgrown and no longer serviceable instinctive tendencies become unconscious. In contrast, the term *repression* is used to note a 'witting' endeavor to accomplish a similar end. Suppression, however, is biological and accounts for complete submergence, especially of those impulses of the all-or-none type which are accompanied by mass-action and strong affective tone. But not all of protopathic sensibility is thus suppressed, part of it being retained and fused into the epicritic sensibility which development has superimposed.

Analogous to phylogenetic development in which higher levels are constantly being achieved through the combined processes of suppression and fusion, we have ontogenetic processes of a similar character accounting for the suppression and fusion of infantile experiences and impulses. Hence in childhood experiences tend to become unwittingly suppressed whenever they involve certain incompatible instinctive impulses; quite as the no longer serviceable biological tendencies of the all-or-none type of mass-reaction are suppressed in the development of critically controlled responses. The precise nature of these suppressions can be studied by reference to the manifestations of instinct, which the author proceeds to classify under the three general heads of (1) self preservation, (2) regeneration, and (3) social cohesion. The first of these exists in two forms: one the appetitive and the other the danger-instincts; the second has likewise two forms: the sexual appetitive and the parental tendencies of solicitude. The third, however, has four aspects, *suggestion*, *sympathy*, *imitation* and *intuition*.

Of the first group the danger-instincts play a large part in the war-neuroses, and may be described under five heads: (1) the reaction of flight with the emotion of fear; flight tending towards an 'ungraduated' total response; (2) the reaction of aggression with the emotion of anger; (3) the reaction of manipulative activity characterized by an absence of affect, as indicated by the marked coolness and lack of fear attending a situation of danger when it is met by any consistent habituated response; (4) the reaction of immobility with complete suppression of action, fear, and pain; completeness being absolutely essential to serviceableness; and (5) the reaction of collapse with the emotion of terror; a combination of both paralysis and tremor arising from an obstruction to any normal instinctive mode of response, and attended by an excess of affect.

The suppressions involved in these various types of response to danger while originally complete, are later modified by partial integration arising under special conditions and needs of the organism; as when an animal first flees until it has removed itself from the source of danger, and only then resorts to the reaction of immobility. "In such a case it is necessary to inhibit a mode of reaction which only a moment earlier has been in full activity. The mechanism of suppression is thus one which must have come into being at a very early stage of animal existence" (p. 68).

The question of the independent existence of suppressed experience in the unconscious is held in suspended judgment. The independent activities of dissociation, as indicated for instance in the *fugue*, are attributed to alternate states of consciousness, without admitting a necessary existential continuity such as Morton Prince has assumed for 'co-consciousness'.

The gregarious instinct, with its affect of *sympathy*, its unwitting *mimesis*, or imitation of the movements of others, and its *intuitions* of another's thoughts, is explained by *suggestion*, to which the author attaches a very special importance. Suggestion is a mechanism of instinct which demonstrates that instinct is not altogether of the all-or-none type; for it is a form of reaction occurring under the conditions of collective or social life whereby graduation and discrimination of responses can be obtained *unwittingly*. Suggestion is thus made to account for the marvels of instinctive resourcefulness found in insects, and though it too, like the mass-response,

tends to be replaced in man by intelligence, it is still functional under special conditions, such as those of hypnosis, sleep, and psycho-neurosis.

With this system of interpretation the author proceeds to deal with various types of psycho-neurosis, which he defines as "attempts, successful and unsuccessful, to restore a balance between instinctive and controlling forces" (p. 119); failure being marked both by an increase in suppressed tendencies and by a weakened control. Special attention is given, first, to anxiety or repression-neuroses, in which the witting attempts at repression are found to be the chief hindrance to recovery because they are unavailing where the impulses are deeply instinctive and subject only to an unwitting suppression. Secondly, hysteria, or substitution-neuroses are considered, and a theory is advanced to explain the characteristic paralyses, contractures and anaesthesias of war victims in terms of the crude protection afforded by partial manifestations of instinctive immobility.

In general, the author finds anxiety-neuroses characteristic of officers, persons of higher intelligence who make a witting though ill-advised attempt to repress their emotions in the presence of danger, whereas hysteria is more frequently found among private soldiers whose training has enhanced their suggestibility. Escape through *dissociation* is the third chief mode of dealing with conflict, and it is found to characterize *phobias, paranoia* with its elaborately rationalized delusions, and *dementia praecox* with its proneness to escape through day-dreams eventuating in hallucinations. The disintegration psychoses such as *mania* with its exaltation and excitement are likewise traced to "different degrees of expression of modes of thought and behavior which, owing to their incompatibility with social traditions and ethical standards, are in health subdued and suppressed" (p. 146).

The last two chapters are given over to a discussion of *regression*, showing how disease of the mind tends to retrace the steps the organism has passed through in the course of its development; and to *sublimation* as a means whereby the energy of conflict can be deflected into channels beneficial to the individual and productive of works of genius.

The appendices, six in number, and occupying nearly two-fifths of the entire volume, serve primarily as a documentation of the ideas and theories set forth in the main body of the book. The whole work might have benefitted if the material had been brought together into a single compact whole; for the initial essay furnishes an anti-climax to the appendices which, obviously, were written from a less comprehensive and a less systematic point of view.

As it stands the book is more 'suggestive' than definitive in its statements, and the mixture of clinical materials with systematic conceptions of interpretation is not always as convincing as one could wish. In particular, the biological theory upon which the author bases his interpretation is open to the serious objection that it is throughout telelogical. The ingenious employment of Head's theories of sensibility and response is therefore perverted by the constant employment of the criterion of *serviceableness* as an explanation of the fundamental operations of suppression. As an illustration of this non-scientific criterion, the author thinks he must meet or set aside a real difficulty in his attribution of the hysterias of warfare to a re-excitation of the all-or-none type of instinctive immobility, because immobility on the battle field would not appear to be a particularly efficacious means of protection.

"To this objection," he tells us, "there are two answers." One is that in the vast majority of cases these protective reactions do not occur on the field of battle, but it may be a few days, it may be months, after the shock, wound, or other event of which they are the sequel. When they occur in this manner they fulfil in great perfection the protective purpose which is the special function of the danger-instincts" (p. 128). Why the 'great perfection' of this fulfillment, after the event has passed and gone, is not alto-

gether obvious to the reviewer. The second answer to the difficulty, applying to those cases where the disability follows immediately, is given in terms of the suppression of fear and pain attending immobility, which makes the individual incapable of realizing the danger to which his throwback exposes him. But the chief point is that the author should find this to be "one of the problems which will have to be settled by any scheme of explanation which can be regarded as satisfactory" (p. 128).

Had the author seen fit to follow Head a little more closely, and interpret these regressions as *compulsory* integrations determined by conditions of situation and affect, instead of insisting that serviceableness must always control and be ever apparent if the individual is at all conscious, we should have had a more scientific statement of the facts of the case, and at the same time one in which the lines of future experimental investigation would have been more clearly laid down.

Cornell University R. M. OGDEN

Manuel de psychologie. By E. B. TITCHENER. Authorized translation by H. Lesage. Paris, 1922. Pp. 571. Price 35 fr. net.

It is curious that, despite the many contributions to experimental psychology by Frenchmen like Binet, Henri, Bourdon, and Piéron, there should not be in their language an introductory text in experimental psychology. The nearest approach to a book of this kind is Wundt's *Eléments de psychologie physiologique*, a translation made in 1885 of the second edition of the *Grundzüge*, which, even if it could ever have served as a textbook, has for many years been hopelessly out of date. It is to remedy this omission that Professor Lesage has now translated Titchener's *Text-Book of Psychology*, which, he says "by virtue of its clearness, its conciseness, its fullness, and its scrupulous distinction of fact and hypothesis, seems to me to be a model of its kind."

Aside from this compliment to the book, and in addition to its value for French students of psychology, the French edition is a contribution to the terminology of psychology at large. If we may trust the opinion of the translator, the psychological vocabulary is not only better established but it is also richer in English than in French. A satisfactory translation, therefore, of a book from the one language to the other must tend to bring the two vocabularies to the same level, and psychology in general gains therefrom. In the present instance there is even a greater gain because Klemm's earlier translation of the *Text-Book* into German as *Lehrbuch der Psychologie* makes possible for the first time in the history of experimental psychology a cross-comparison in three languages of the terms employed in one and the same book.

Professor Lesage's translation is entirely satisfactory. He brought to his task both a full realization of its difficulties on the side of terminology, and a feeling for style and *l'expression heureuse*. He has sought the assistance of Bourdon and Titchener in certain technical and idiomatic passages; wherever he is in doubt or dissatisfied with a term he has placed the English equivalent within parentheses; and where he has been forced to make a neologism he has based it on the English word. He has added no new references to the experimental literature, but Titchener himself has in a new preface indicated some of the more important lines of development during the last decade. The Subject-Index has, unfortunately, not been translated. The only error we have thus far discovered is the omission of a sentence on p. 393, which destroys the sense of the passage.

H. P. W.

General Psychology in Terms of Behavior. By STEVENSON SMITH and EDWIN R. GUTHRIE. New York: Appleton, 1921. Pp. xii, 270.

The teacher of psychology to-day is not (as a rule) content to teach psychology as psychology. Owing to his own enthusiasm and to popular demand, he sometimes endeavors to present his subject so that it will be all-explanatory and all-inclusive; it appears necessary to him that it should be easily applicable to the social sciences. When a psychologist, therefore, states in positive terms *why* individuals and groups conduct themselves in certain ways, he has prepared fertile ground for the educator, the economist and the sociologist. He has vindicated himself and his discipline; he has secured for himself academic and popular approval.

A psychologist, then, bent upon such a purpose must have a text-book which will assist him in carrying out this point of view. Such a text, it seems to the reviewer, is *General Psychology in Terms of Behavior.* The purpose of this work, as stated in the preface, "is to describe man's original nature, the way in which this nature is altered by use, and the common modes of individual and social behavior that result." The facts are selected and presented so that the student may have a "systematic explanation of his own conduct and that of his fellows."

Psychology is the science of behavior, we are told, that "takes the common sense view that any animal is a physical object in a world of physical objects. It assumes that all these objects act upon each other in the ways described by physics, chemistry and physiology. It considers man's behavior as a physical event that can be analyzed into bodily movements." If this study of the bodily movements is to constitute a science, we might ask why it is necessary to consider that all responses have a certain utility, which may or may not be apparent? "Although we may usually expect to find some use served by every response, there are many cases of inappropriate reactions. For example, the instinct that leads a dog to run barking beside the front wheels of an automobile seems to serve no useful purpose either to dog or driver. If we remember that the dog is the descendant of the wolf and that the wolf in killing his prey must depend upon the pack for assistance, it is clear that there was a time when this reaction was of service." This attempt to explain all behavior cannot be undertaken, in the opinion of the writer, at the present time. Surely, if "psychology is the science of behavior" the study of behavior must follow the methods and the exactitude employed in the other physical sciences to which it claims relationship.

Again, it is stated that "a complete understanding of behavior always involves an analysis in terms of stimulus-response mechanisms." Later, however, in treating two opposed reaction tendencies, the author tells us that "introspection usually detects either a bodily change or a change of external stimulation as an antecedent of voluntary movement." Why is it necessary, in this case, to resort to introspection?

The usual topics considered by the behaviorist are treated in five chapters entitled: The Elements of Behavior, Instinct, Learning, Coenotropes (or Common Habits) and Perception. In the sixth and seventh chapters, the topics of Human Motives and Social Psychology are taken up, and an attempt is made to describe phenomena of this variety in terms of the stimulus-response mechanisms.

In the Appendix of this text, we are informed that "a description of mind in terms of stimulation, neural action, and responses is by no means the only one in use. There is another language, which the individual may employ in describing the world as his own experience, and which includes terms that are useless in a description of behavior. This is the language of consciousness, and its importance is established by its popularity." The student has already been introduced to an objective psychology which has been quite adequate, in the opinion of the authors, to explain all behavior.

To such a student, then, psychology must be a study of behavior, and it would seem that the introduction of short paragraphs which deal with the topics of Consciousness, Sensation, Emotion and Affection, Images, Association of Ideas, Imagination, Attention, Perception and the Unconscious (they are given seventeen pages) must result in nothing but actual confusion for the student. It might also be noticed that popularity alone does not account for the importance and value of the "language of consciousness." Scientific theories have persisted not on account of their popularity, but in spite of prevailing modes of thought.

In an elementary text, of course, many theories may be presented without stating the evidence upon which these theories are based. To this one would not object; but when theories are presented for which there is no scientific evidence, objection is perfectly valid. The reviewer objects therefore to many of the theories in explanation of individual and social conduct presented in this work on the grounds that they are mere matters of opinion. Theories of this sort cannot constitute a *science* of behavior which will give the student, or anyone else, "a systematic explanation of his own conduct and of his fellows."

Cornell University SETH WAKEMAN

The Psychology of Learning. By W. H. PYLE. Baltimore, Warwick & York, Inc. 1921. pp. 308.

In the preface to this little book the author says that "he has tried to state every thing that is known about learning." How nearly this programme, rather ambitious in itself, finds fulfillment will depend to a considerable degree on the definition given to learning. Learning turns out to be a purely practical affair; it is what we learn in the way of habits formed and knowledge acquired, *i.e.*, organised experience. The essential thing in the one is *movement*, in the other, *idea*. We are told also that "learning consists, psychologically, *in building up and establishing preferential routes in the nervous system.*" This hypothetical neural basis is stated or implied throughout. The initial connections result from trial and error and from simultaneous experience, respectively, in the two forms of learning. If we go to the chapter on ideational learning and inquire more closely for a theory of learning, we read: "By ideational learning we mean acquiring knowledge." In knowledge "the important thing is *sequence of ideas.*" We seem to be running in a circle. The definitions, however, may help. "The meaning given to each of these terms is a simple common-sense one." ....."Ideas are the conscious processes representative of objects of revival or secondary experience." Knowledge is "the name for connected ideas:" at least one term of the connected pair must be an idea. Ideas get connected if the "processes take place together in the brain or come in close succession." Hence the "*inner world of ideas*" is orderly because "the outer world is lawful, orderly, systematic." But there is more in the process: for "things which we have experienced together may have for us no useful connection." "We therefore *reorganise* the world by thinking over our experience in the relations that are important to us." The reader halts at the 'we' and the 'relations'; for these are surely new factors. We learn nothing about the 'we'; but we are told that the causal relation is the most important one, since "causal sequences exist in nature." We had supposed that 'cause and effect' was a man-made notion. Three pages farther on we find that "the most important thing about an idea is its meaning;" that "the meaning of an idea is another idea closely associated with it: one that represents some characteristic, some relation, some use." We may pass by the implied infinite regression and ask anew, What is knowledge? We find now that "most of our knowledge is knowledge of meanings; that our progress in knowledge consists in learning new meanings." We get

nothing more definite. All this may be a theory of learning, but it can scarcely be said to exhaust all that is extant on the subject.

We might in a similar way go into an analysis of other chapters, and in every case we should find that the book before us is not exhaustive. In fact the card-sorting experiments made by the author form the backbone and skeleton of the work. This statement is not intended as a negative criticism except in the light of the opening sentence of the preface. In every one of the fourteen chapters some topic receives consideration in a rather simple, elementary way that reflects, what the author so often professes, a common-sense point of view.

A fairly complete list of references follows every chapter, and there is a general bibliography at the end of the book.

The Child and His School. By GERTRUDE HARTMAN. New York, E. P. Dutton & Co. 1922. pp. xiii, 248.

This book grew out of the effort on the part of the author to compile a bibliography which should serve the needs of those teachers who are interested, experimentally, in curriculum-making, and out of the conclusion that such a list of titles stripped bare of any unifying principle is next to worthless. In its outward aspect it is very largely a compendium of quotations, for the most part from the writings of John Dewey, of whom the author is an enthusiastic follower.

The system makes a progressively developing social order the object or aim of education, and the assumptions of a dynamic behaviorism the means for its realisation. But there is more here than Woodworth, to whom the author refers her readers, would admit; consciousness appears in the part ("a superior device for securing adaptation") to which the functionalists assign it, and agreeableness and disagreeableness appear as the agents which select from among the several adaptations. Hence we find that "social considerations determine the end toward which adaptations are to be directed and supply us with the subject content of education; psychological processes dictate the method by which our aim may be accomplished." This emphasis upon the consciously adapted activity as means robs the subject matter, as such, of any claim to an independent existence or to a consideration in its own right.

The entire school-programme is worked over, with general indications of the way in which the several subject-matters which grew out of social demands may serve to solve the problems of education instead of themselves being the problems. Finally, the changes demanded by such a system in the work of the teacher, in the measures of success, and in school-equipment receive discussion. On the whole the book forms a fairly complete working-out of Dewey's philosophy of education, and will for that reason be of interest to educators. In so far as such efforts tend to free us from our bondage to subject-matter and to emphasise attitude and process, we may wish them all success; as a practical solution of a practical problem, trial alone can decide the merits of this particular essay. The book carries, beside sectional references, a bibliography of over a thousand entries, many of which are duplicates due to their topical arrangement.

Nutrition and Growth in Children. By WILLIAM R. P. EMERSON, M.D. New York & London, D. Appleton and Co. 1922. pp. xxix, 342.

The book deals, primarily, with the problems of the undernourished child and with the nutritional class as an agency for their solution. Except for the technical terms, the layman can understand the book and may get helpful hints from it; especially from the charts and photographs. But

the book is in no way intended for the layman; many other sources are available which give him what he needs to know in more convenient form. The book is for the clinician, since malnutrition is a disease that demands the services of the expert as much as does any other ailment.

The author's main thesis is that malnutrition results, invariably, from the various combinations of part or all of a limited number of factors all of which lie within our power to control. These factors are home control, food habits, health habits, physical defects, especially obstructions to breathing, and overfatigue. Contrary to some writers, the author does not place diet at the top of the list; "it is not the first or chief cause of malnutrition, but actually fourth in the list and shares even this place with faulty food habits." The cooperation of the home, the school, the medical profession and the child's own interest is necessary for their control. The clinician diagnoses, arouses interest and prescribes; the burden of reform, aside from physical defects, falls on the home with the cooperation of the school.

The fundamental measure of nutrition is the relation of weight to actual height quite independently of any norms of height, though these are not wholly disregarded. Such norms as we have for height and especially for weight are so low as to be unreliable, since they include the undernourished who make up a considerable percentage of the population. Accordingly the author presents a new set of norms. The mental life of the child may be symptomatic; hence we infer that mental retardation or even reversion parallels the physical state. A somewhat modified form of the Stanford Revision of the Binet-Simon test series, given without control, enables the mother in the home to measure the intellectual status of the child. We infer also that in the interpretation of the IQ we must take into account the state of nutrition of the subject. This very brief chapter reveals the usual weakness of the man of medicine when he writes on subjects that touch psychology.

The book is primarily a book of professional propaganda, and as such should render service in a field where much remains to be done.

Psychanalysis in the Class Room. By GEORGE H. GREEN. New York and London, G. P. Putnam's Sons. 1922. pp. 272.

With a greater or less show of reason the teacher evidences a distrust of psychology, which distrust will break down under the forces of the practical results to follow from the application of the principles of psychanalysis. "The teacher who is able to make himself acquainted with them will discover that they help him to explain much in the mental life of children which previously puzzled him, and which appeared incapable of explanation. . . . It enables the teacher to understand himself and to realise that a great many difficulties that occur in his work are the result of tendencies in himself."

It is important, if psychanalysis is to furnish this insight into the mind of the child and thus to render service in the resolution of the difficulties that beset education, that the teacher should know where to look for the disturbing factors, what to look for, and what to do when she finds them. Those traits, activities and what-not which interfere with the progress of the child, not alone in his school-work but also in his more complete development, reveal themselves in day-dreams, play, dreams, word-associations, interests, slips, accidents, omissions, and in the phenomena of introversion, extraversion and identification. The factors which actually make for interference are unfulfilled wishes, impulses, interests, and modes of response actuated by the unconscious activity of instincts that find less than their normal expression in the environment in which the individual leads his daily life; in a word, instinctive wishes. The most common bases

of these wishes are the instincts of domination, homage, subjection, self-assertion, possession, sex, and the gregarious instinct; self-assertion, domination and possession would seem to be the most important. The remedy lies in the exposure of the submerged wish, especially when it grows out of some early experience of an unpleasant sort, and sometimes in the fulfillment of the wish; but the more usual and wholesome remedy lies in sublimation. "The eradication of the instinctive wishes would mean the eradication of the whole of the pupil's motives; not motives for daydreaming only, but for doing anything whatsoever. . . . In the psychanalytic view the chief aim of education is to discover the nature of the deep instinctive motives of children and to train them from egoistic levels of expression to altruistic levels."

The book reveals many features of the Freudian system, with its emphasis on the unconscious aspect of mind with interests that differ from those of the conscious mind; but the author lays greater stress on the other instincts, for which he accepts the terminology of MacDougall, and less on sex. To those who accept the doctrine of complexes in all its latitude, the book will come as a welcome extension of it, specifically, to the problems of education; to those who do not believe that every dream, every act of play, every word-association, every slip, every interest in Cinderella, Charlie Chaplin and fairy stories, every accident, every act of kindness or tap on the table means an attempt to escape from the world that is into an ideal world in which all the instinctive wishes find fulfilment, the book will appeal less strongly. There is little that is new in the way of fundamental concepts.

The Education of Behavior. By I. B. Saxby. London, University of London Press, Ltd. 1921. pp. vii, 248.

The author accepts the definition of education offered by Herbert Spencer, which in general terms is "preparation for adult life; and this in turn may be described as preparation for efficient citizenship." This attitude, together with his acceptance of the instincts and emotions of MacDougall, the sentiments of Shand, and the complexes of Freud, gives the key to the book; "it is an attempt to bring our present knowledge of psychology to bear on the problems of behavior which have to be faced by those who are in charge of boys and girls during their adolescence." Of the two factors, heredity and environment, which determine behavior, heredity supplies the 'vital energy,' but environment supplies the moulds that shape the patterns; "the natural endowments of the child present as it were the sum total of his possibilities, but it depends on environment how they develop."

The urges or sources of energy for action spring from the inherited tendencies toward self-preservation, race-preservation or a combination of these. More specifically, the inborn tendencies to action may be classed under reflexes, impulses (the usual instincts), sentiments and complexes. The sentiments which arise as the impulses organise about some object of thought are "really *centers of potential activity* of which we become aware through our likes and dislikes." These, with the complexes which are unconscious sentiments, form the "mental systems which make for consistency in *acquired* forms of behavior." They are on that account basic for habits, emotions, and sympathy as well as for all that goes to make up character. Thus far we have a group of systems played upon by environment in the form of percepts, or as perceived, and the whole seems consistent although purely conceptual. But when we come to consider character, other forces demand admittance. We now have in addition intelligence, dispositions, a general attitude towards life, and will power. Intelligence

we are told, we know about already; disposition becomes a function of impulses; attitude under its two aspects, temperament and perseveration, becomes a matter of physiology; and "will power, *i.e.*, the energy that makes it possible for us to overcome resistance within ourselves, is in some way derived from our self-regarding sentiment." We must not, for all this, view these systems as blind mechanisms to be played upon indifferently by environment. Not only do the impulses have ends, which need imply nothing more than teleology, but the 'self' steps in to change things in all sorts of ways; it prevents overflows of energy by diverting it to other forms of activity, it desires to avoid or wants to overcome, and the needs of the 'self' determine the strength and permanence of sentiments. This view is yet in harmony with the major thesis; for "in any particular case the final idea of the 'self' is the combined product of the innate possibilities of the individual and of the pressure which has been exerted by his environment."

When one approaches a problem of utility, of ends to be attained, every means to that end become legitimate. The author in this book assumes the concepts of MacDougall, of Shand and of Freud, and by modifying here and there, as in the case of emotion and the complexes, works the lot into a unified whole. It is a pretty display of logic as applied to words but, even if it should 'work,' that would prove nothing of the truth or falsity of the concepts. The place of such conceptual constructions is in the world of values.

How to Measure in Education. By WILLIAM A. MCCALL, New York, The Macmillan Co. 1922. pp. xii, 416.

There is no need to attempt even a summary of the contents of these more than four hundred pages of topical material. A great deal is here, but there is much that we miss. As we read we wonder why the book need be more than one-half its present bulk; that is, as we read with the impress of the title and of Chap. I on us. We find the answer in the preface; the author means to tell not only the How but also the Why. The wisdom of the mixture is not apparent. He who needs the Why is scarcely ready for the How, and he who wants the How gets lost in the Why, so that if he is at all serious he will turn to other sources where the material is ready to hand.

The book strikes the reader as extremely elementary; it is as if the teachers had become the victims of their own 'soft pedagogy'. It is difficult enough, perhaps, for the very beginner; but with a little pruning here and there some of the more involved and even of the more fundamental concepts could have found a place. But what strikes one most is the divergence between what is, at least tacitly, promised in the theses of Chap. I and what one actually finds in the rest of the book. The ideals of Chap. I can not now and in all probability never will be realised. For example, it may be true that we have no element showing a qualitative dimension that does not at the same time show a quantitative one; but this does not mean that the qualitative may not vary independently of the quantitative aspect. It is less harm, however, to promise more for the future than we can give at the present time than it is to give less than we can. If we are going to measure, there is just one set of tools at our disposal,—the mathematical. When the author comes to the matter of weighting, he tells us that weight is a function of variability; yet he assigns weights to his own scores on the basis of some vague notion of their importance and "because it was a saving of labor." Such procedure can never lead to exact results, nor can the plea of simplicity justify it.

On the other hand, the book gives information about how to use a great many of the measures which educators have used; and where they

have used two types of measure for the same end, it gives the reasons in non-mathematical terms for their choice, together with the advantages and disadvantages of each type. Another good feature is the numerical arrangement of points on any topic according to their order of development.

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PSYCHOLOGICAL PERIODICALS

Zeits. f. Psychologie, Bd. lxxxviii., Heft 3 bis 5. G. RIES. 'Untersuchungen über die Sicherheit der Aussage.' [Reports experiments on subjective assurance; the materials were for the most part visual objects, meaningless and meaningful, of graded complexity, tachistoscopically exposed; single series were taken with words brought in from a distance (Wiegand) and with odors. The experiments fell into three groups: in the first, the observers stated what they had cognised at a single exposure; in the second, they judged whether the given stimulus was identical with (or similar to) a previous stimulus; in the third, they were asked to identify odors. The results of the first group showed that subjective assurance is conditioned on exclusiveness of reproduction (less upon the other Müllerian factors of promptness and insistence), on fulness of ideation, on meaning and associative connection; more generally, upon attention and readiness of residua. The second group furnished nothing new. The third group brought out the fact that the coexistence of sensation with idea of name is not always adequate to assurance; an express comparison might be made of sensation with memory image.] Anhang: F. SCHUMANN. 'Das Erkennungsurteil.' [The cognition of a visually presented word depends upon the cooperation of residua, the feeling of familiarity (not invariable), the arousal of the auditory-motor word-image, and the conviction that this image corresponds with the visual presentation. Experiments with odors prove that this conviction, in turn, depends upon the coincidence of the residua aroused by stimulus and name. It may consist, experientially, in increased insistence of quality, feeling of familiarity, satisfied expectation, the formation of a more unitary complex; or it may contain an as yet unidentified component.] T. SCHJELDERUP-EBBE. 'Beiträge zur Sozialpsychologie des Haushuhns.' [Every bird has its established place in a social hierarchy, determined ordinarily by combat, and carrying with it the privilege of harrying (pecking) subordinates.] Anhang: D. KATZ. 'Tierpsychologie und Soziologie des Menschen.' [The preceding article gives various suggestions toward an experimental sociology. There is, *e.g.*, in man also, a congenital tendency to form ordered groups: cf. schoolboys, sportsmen, intellectual cliques.] V. HAECKER und T. ZIEHEN. 'Ueber die Erblichkeit der musikalischen Begabung: nebst allgemeinen methodologischen Bemerkungen über die psychische Vererbung,' i. [First part of an investigation based on questionary returns (595 replies, relating to at least 5000 persons). Discusses terminology; begins the discussion of results. Thus, discordant marriages (plus-minus, minus-plus) show a far higher percentage of plus and double-plus than of minus and double-minus descendants. Gives the evidence for and against the occurrence of the various biological types of inheritance.] Literaturbericht. E. R. JAENSCH. 'Zur Richtigstellung und Ergänzung.' [Explains the intercurrence of phenomena of macropsia in the work of P. Busse, vol. 84.] Gesellschaft für experimentelle Psychologie.

Bd. lxxxviii., Heft 6. J. PLASSMANN. 'Psychologische Erfahrungen mit einem Zeitsignal.' [A study of the reception of wireless time-signals from the Eiffel Tower. As against the average of all signals, the three longs are taken too early, the long-short, and especially the long-long-short, are taken too late. In the former case, only the personal equation is involved; in the latter, the observer is prejudiced; he looks for a final long. Experiments in the laboratory are needed.—In the records, the figures o,

1, 2 are overdetermined; the figures 3, 4, 5, 6, 7, 8, 9 are underdetermined.] K. SAPPER. 'Physikalische Betrachtungen zum Problem der Wirkung psychischer Faktoren auf materielle Systeme.' [Physics can raise no objection to an operation of a psychical or vitalistic factor upon material particles such that the direction (but not the velocity) of their movement is changed. Since, however, this operation has strict limitations, and since every mechanical force may be analysed into an accelerating and a directive component, it is better to regard the psychical factor as operating directly on the directive component of the force and thus only indirectly on the material movement.] J. HIRSCH. 'Ueber traditionellen Speisenabschau: ein Beitrag zur genetischen Gefühlspsychologie.' [Traditional aversion from certain forms of food cannot be explained teleologically; the line of development runs rather from prohibition through avoidance to aversion. The prohibition may arise through conflict of cults (pork among the Jews, horse-meat among the modern Germans); usually, however, it goes back to primitive magic (totem-animals, soul-animals, protective animals). A similar magical origin accounts for our aversion from the flesh of animals that have died a natural death, from blood, from food that contains hairs, from the remains of food left by another on his plate, etc., etc. The reasons for such aversion offered by civilised man are merely rationalisations; they may serve to maintain or intensify the tradition, but they could never have originated it. The passage from avoidance to aversion is accounted for, psychologically, by the close relation of reverence to aversion.] Literaturbericht.

Bd. lxxxix., Heft 1 bis 3. W. PETERS. 'Das Intelligenzproblem und die Intelligenzforschung: eine Auseinandersetzung mit William Stern.' [Critical review of Stern's *Die Intelligenz der Kinder und Jugendlichen und Methoden ihrer Untersuchung*, 1920. (1) In place of a theory of intelligence Stern offers a definition, which theoretically is altogether uncertain and practically is useless to the psychologist. (2) In his account of the Binet method Stern gives no general survey of corrections, changes, extensions, and makes no attempt to supplement the determination of status by a determination of cause. (3) Experiment and observation can be applied alike both to reactive and to spontaneous behavior, since there is no essential difference between these forms of intellectual expression. (4) Stern neglects statistical methods. In particular, there is no evidence whatever for the constancy of the IQ in the case of intellectually backward children; theory and fact both speak for a gradual decrease. (5) Both the teleological nature of normality and the doctrine of qualitative difference in abnormality are assumptions only.] H. HENNING. 'Assoziationsgesetz und Geruchsgedächtnis.' [Discusses, chiefly with reference to smell, the differences in association and memory between the qualities of the higher and those of the lower senses. Thus (1) palpable olfactory images and abstract olfactory surrogate ideas are not associatively equivalent; the latter stand generally on a par with meaningless syllables, words, figures; the former are far superior. (2) An odor presented as object-odor in the context of a total situation has an immense associative power; an odor presented as datum-odor, say with a meaningless syllable, has less associative power than another syllable. (3) The primary meaning of an odor depends upon its object-character within an unitary complex (total situation); its concrete actuality; and congruence of the factors in the situation (habituation).] A. RIEKEL. 'Psychologische Versuche an Hühnern: Ueber den Vergleichsvorgang und damit zusammenhängende Fragen, besonders auch über das Farbensehen der Hühner.' [Experiments, with simultaneous and successive presentation of stimuli, prove that the process of comparison depends upon 'transitional' experiences; the result is confirmed by experiments on young children, before the age of naming, and itself confirms Jaensch's work on eidetics, while it makes against Köhler's theory of

structural function.—The birds are sensitive to blue, but far less sensitive than is man.] E. R. JAENSCH. 'Ueber den Aufbau der Wahrnehmungswelt und ihre Struktur im Jugendalter.' v. E. R. Jaensch (mit einigen Versuchen von H. Freiling). 'Ueber Raumverlagerung und die Beziehung von Raumwahrnehmung und Handeln.' [Transpositions of various kinds, arising spontaneously in eidetic images of bidimensional objects, are conditioned upon successive antagonistic attentional impulses; the phenomena are therefore akin to that of the horopter deviation (transposition in the third dimension). Incidentally, Mach's problem of symmetry is thus brought nearer solution.—These transpositions are valuable in the forestages of thinking: *e.g.*, in recognition, in the thought-experiment (in Mach's sense).—Since eidetics are able to produce transpositions, not only in their images but sometimes also in the actual objects of perception, in relation to a predetermined end, there is no reason to adopt Köhler's 'insight' as an explanatory concept in chimpanzee psychology; optical-dynamic processes are sufficient.] Literaturbericht.

Bd. lxxxix., Heft 4 bis 6. F. HILLEBRAND. 'Zur Theorie der stroboskopischen Bewegungen,' i. [The Wertheimer phenomenon cannot be accounted for by retinal processes alone; it requires the cooperation of that factor, known as the vehicle of 'absolute' localisation, which was introduced to explain why visual objects remain at rest, under voluntary movement of regard, although the images on the retina have shifted. This factor depends on attention; the retinal space-values are revalued (compensated) by transference of the point of attention (*Aufmerksamkeitsort*); and the effect of attention is complete distinctness of the terminal point of the intended movement. Stroboscopic movement is the expression of a passage through all stages between null and total revaluation; it is the correlate of the gradual process of revaluation.—This instalment discusses the circumstances which influence the mode of stroboscopic movement, and stroboscopic movement under fixed regard.] V. HAECKER und T. ZIEHEN. 'Ueber die Erblichkeit der musikalischen Begabung,' ii. [Examines the distribution of the descendants of discordant marriages among the various biological types. Concludes that males are in general somewhat more receptive than females to transmitted musical endowment, and that the mother is in general somewhat more effective than the father for such transmission. Begins the discussion of the result of positively concordant marriages.] J. LINDWORSKY. 'Umrisskizze zu einer theoretischen Psychologie.' [(1) There are two kinds of psychical processes. The one (successive contrast, spontaneous reproduction, the arousal of sense-feeling) shows indifference of content, is physiologically conditioned, and is palpable; the other (the apprehension of relation, conation) is not indifferent as regards content, is psychologically conditioned, and is impalpable. The second class brings psychology into relation with the mental sciences. (2) The general law of the conscious life is that all conscious phenomena take place in an experiencing subject. There are also special laws: the law of correlation (every psychophysical process has its correlated psychical); the law of continuity; the law of apprehension of relation; the law of conation (reciprocal relation of conation and value); the law of attentional effect (the will can, within limits, enhance the intensity of ideas).] Literaturbericht. G. A. ROEMER. 'Berichtigung.' [Reply to Stern's review of Rorschach's *Psychodiagnostik*.]

Bd. xc., Heft 1 und 2. F. HILLEBRAND. 'Zur Theorie der stroboskopischen Bewegungen,' ii. [Discusses, under various heads, the dependence of stroboscopic movement on change of magnitude of excitation; brings direct proof of the shift of the field of vision in stroboscopic movements; summarises the theory. Two appendices deal (1) with preceding theories (Wertheimer, Linke, Koffka) and (2) with the author's experi-

mental technique.] E. RUBIN. 'Zur Psychophysik der Geradheit.' [(1) There are likenesses and differences between physical and psychological straightness. Thus, one can no more see two straight lines connecting the same two points than one can construct such lines geometrically; lines to be seen apart must diverge 30″, but lines to be seen both as straight must not diverge by more than 22″. On the other hand, two lines may appear of the same length, and yet one may look straight and the other just noticeably curved. (2) Arrangements of discrete points etc. may give the appearance of straightness; here, however, the relation of grouping (*Zusammengehörigkeit*), which has no physical parallel, plays a large part. Various illustrative figures are given. (3) Attitudes have a direct influence upon the limen of curvature as determined by minimal changes; in the method of constant stimuli these attitudes are replaced by tendencies to bend the line to right or left and by direction of attention. The limens yielded by the second method are the smaller.] T. SCHJELDERUP-EBBE. 'Soziale Verhältnisse bei Vögeln.' [Observations on a large number of species confirm the writer's previous report: always a social order of rank is formed, whether between birds of the same species or in mixed groups.] Literaturbericht. G. E. MUELLER. 'Nachruf.' [Notice of the work of W. Baade.] Dr. HAUSCHILD. 'Notiz.' [Exposition of handwriting, drawings, etc., of psychopathological subjects.]

Bd. xc., Heft 3 bis 6. H. ROTHSCHILD. 'Untersuchungen über die sogenannten Zöllnerschen anorthoskopischen Zerrbilder.' [If visual figures are moved horizontally behind a vertical slit, at high speeds they look narrower and at low speeds broader than they appear under ordinary conditions (Zöllner). The writer finds that this effect cannot depend upon eye-movement; experiments with unequivocal and with equivocal figures prove alike that the chief factors are difference between figure and ground, and contour of figure. The observation that a figure, though narrowed, may still look wider than the slit through which it is seen is not explained.] J. BAPPERT. 'Neue Untersuchungen zum Problem des Verhältnisses von Akkommodation und Konvergenz zur Wahrnehmung der Tiefe.' [Experiments with edges, rods and luminous points show that neither state nor change of state of accommodation and convergence, nor yet the impulse to such change, is a direct factor in the perception of depth; accommodation and convergence are, however, directly concerned with apparent magnitude. Even when a change of accommodation and convergence is perceived, the perception is often wholly indeterminate as regards distance; and its voluntary initiation does not give it determinateness.] V. HAECKER und T. ZIEHEN. 'Ueber die Erblichkeit der musikalischen Begabung,' iii. [Concludes the exposition of facts concerning descendants (negatively concordant marriages) and ascendants; there is general agreement between the two sets of results. Proceeds to discuss components, development, and correlations of musical endowment.—The whole material suggests a modified *Pisum*-type; evidence of extra-Mendelian modes is disappearingly small. It is surprising that the negatively concordant marriages yield relatively numerous plus and even double-plus descendants; the possibilities of explanation are set forth at length. The curve of development shows two peaks. In the male sex there is correlation of musical with artistic (graphic) and still more definitely with poetic endowment; the case for mathematics is not made out.] M. FLOORS. 'Ueber den Einfluss der Grössenvariierung bei Gedächtnisleistungen.' [Experiments undertaken to determine the influence of variation of the size of figures upon their recognition and upon their capacity to reproduce, and to be reproduced by, associated syllables. The best results are obtained when the figures impressed and tested remain of the same size. If the figures tested are of different size from those impressed, the best results are obtained when the figures impressed have themselves been of various sizes. If the test is made by way of the

associated syllables, the best results are obtained when the corresponding figures have remained of constant size during impression. With adults, large figures are more favorable for impression than small (confirmation of P. Meyer's thesis).] R. HENNIG. 'Neue Untersuchungen zu einem Fall von abnormem Datengedächtnis.' [Cf. vols. 10 and 49. Instances of retentiveness; sympathetic and unsympathetic dates; mnemonic devices.] Literaturbericht. G. E. MUELLER. 'Gesellschaft für experimentelle Psychologie.'

Archives de psychologie, tome xviii., nos. 1, 2. E. CLAPARÈDE. 'Théodore Flournoy, sa vie et son oeuvre, 1854—1920: avec un portrait.' T. FLOURNOY. 'L'idée centrale de la critique de la raison pure.' [From notes written in 1884 or 1885 for a lecture-course. The heart of the Critique is the transcendental analytics, whose two books give a rational justification of the application to scientific objects of our a priori notions, and the development of the general principles grounded in these notions.] M. J. LARGUIER DES BANCELS. 'L'abîme de Pascal.' [The story of Pascal's 'precipice' need not be doubted. Janet has described a similar case; E. Jones has analysed a closely related phobia; and Pascal's niece gives details of his early childhood which may easily be brought into connection with his affliction.] J. PIAGET. 'Une forme verbale de la comparaison chez l'enfant: un cas de transition entre le jugement prédicatif et le jugement de relation.' [Two forms of the test "A is fairer than B but darker than C: who is darker—B or C?" were given to two groups (37, 20) of primary schoolboys aged 7 to 14. It appears that the earliest judgments are predicative (judgments of classes). Between these and judgments of relation proper lie intermediate forms, in which relation is introduced within the classes (prerelation), while the classes themselves multiply and interfere. The writer finds that the form of judgment correlates with (acts and reacts upon) the field of attention.] Recueil de faits: documents et discussions. C. WERNER. 'Réunions des philosophes de la suisse romande: xv. Lausanne, 20 juin 1920; xvi. Lausanne, 12 juin 1921.' E. CLAPARÈDE. 'Ire et IIme conférences internationales d'orientation professionnelle: I. Genève, 27-28 septembre 1920; II. Barcelone, 28-30 septembre 1921.' Bibliographie. Necrologie. [A. Leclère, W. Wundt, etc.] Notes diverses.

Psychological Review. Vol. xxviii., no. 3. 'In Memory of Wilhelm Wundt, by his American students.' [Brief notes by seventeen students, with portrait.] J. A. MELROSE. 'The Structure of Animal Learning.' [The lowest animal learning begins with organic fixation, the defining and fixing of inherited behavior by practice. This is followed by organic spatial accommodation, by learning through the repetition of direct sense-stimuli and the assimilation of the result; and this, in turn, by organic choice or adaptation to functional meaning, which is learning through the repetition and shortening of serial reaction. Higher animal learning begins, in animals endowed with smell and sight, by way of organic association, the fore-stage of association by contiguity; odor becomes connected with food, and so a cross-connection is made and a new term of adaptive value is added. This is followed, in the case of animals with sight, by use of the organic concept; an animal in a new situation may respond in a way that shows the influence of past experience with like concretes and yet that is not wholly determined by such experience. Finally, the highest animals below man possess the organic judgment; their behavior shows association of complexes, of relations; there is close accommodation to the general problems of the environment. Throughout the entire field of learning we thus find three constants: repetition of stimuli and response, synthetic assimilation of effects, and the projection of the integrated result of these two in behavior.] F. T. RUSSELL. 'A Poet's Portrayal of Emotion.' [Discusses and illustrates Browning's treatment of disgust, envy, fear, anger, grief, joy, pity, shame and

adoration. The poet shows how the ephemeral and discordant emotions of psychology may be transformed into sentiments, convictions and intelligent behavior worthy of the dignity of human life.]

Vol. xxviii., no. 4. H. B. ENGLISH. 'Dynamic Psychology and the Problem of Motivation.' [Pleads for a theory of motivation broader than hedonism or the instinct-theory. The dynamics of human behavior reside not only in the innate mechanisms but also in any relatively permanent psychophysical disposition (habits).] H. C. WARREN. 'Psychology and the Central Nervous System.' [Psychology deals with stimulation of organisms by their environment, central nervous activity resulting therefrom, and responsive activity of whatever sort consequent on this central activity. Since the essential feature of the stimulus-response process is constituted by the operations in the central part of the nervous arc, behavior and introspection are coordinate methods of psychological enquiry. To these the neurological method must be added. The double-aspect theory provides the best regulative principle for psychology.] A. F. BRONNER. 'Apperceptive Abilities.' [Apperception, as "the 'sizing-up of a situation,' the interpretation of all the aspects, each in the light of the other," is a particularised ability, among both the mentally normal and the defective, that has prognostic value and important social implications. The Pictorial Completion test measures little else than apperception.] A. B. WOLFE. 'The Motivation of Radicalism.' [The individual who suffers from obstructed interests has three ways in which to readjust himself to his environment: repression, substitution and transference, and reinforcement. The first has no connection with radicalism. The second, commonly called sublimation, may lead either to a symbolical conservatism or to a symbolical radicalism; this radicalism is usually derived from egotistic interest, and is likely to be superficial, emotional, ineffective. Reinforcement alone gives drive and consistency to radical movements; it may make radicalism, as the result of balked social sympathy, a powerful social force.] G. M. STRATTON. 'The Control of Another Person by Obscure Signs.' [Tests of the muscle-reading sort, with and without contact (slack watch-chain from hand to hand), made on a professional exhibitor. When touch was excluded visual aid was received from obscure and fleeting signs given unintentionally by the 'guide;' when all such visual cues were also excluded, the successes dropped to the number indicated by pure chance.]

Vol. xxviii., no. 5. W. MCDOUGALL. 'Belief as a Derived Emotion.' [Belief is neither a primary nor a compound emotion, but one of the 'derived' emotions which may accompany and qualify the operation of any strong desire; it thus belongs to the group of which confidence, hope, anxiety, despondency, despair and regret are the principal members. Indeed, what confidence is on the place of practical bodily striving, that is belief on the purely intellectual plane. In its fullest sense, belief is always preceded by doubt, deliberation, and choice of means.] J. R. KANTOR. 'How do we Acquire our Basic Reactions?' [Maintains, against any doctrine of innate powers, that "each stage of our development represents the acquisition of response systems, by means of the modification of previously acquired reaction systems and through the instrumentality of newer forms of stimulation, that is to say, through contact with new types of objects and conditions." The contact (stimulation) may be physical, personal, cultural; its process may be casual or deliberate. The author supports his view by an analysis of various reaction types: adaptive, compensatory, manipulative, expressive, approbative, accommodative, etc.] M. W. CALKINS. 'Fact and Inference in Raymond Wheeler's Doctrine of Will and Self-Activity.' [The records bear unequivocal testimony to the occurrence of a consciousness of self which their writers do not and could not analyse into sensation-complexes and imagery. The self in volition is thus both kinaesthetically and 'actively' conscious.] E. L. THORNDIKE. 'The Correlation be-

tween Interests and Abilities in College Courses.' [Further inferences from Bridges' data (July, 1920) corroborate the idea of a very close relation between the order of an individual's interest and the order of his abilities.] C. L. HULL. 'A Device for Determining Coefficients of Partial Correlation.' [Consists of scaled sum-and-difference chart of Kelley's *A* and *B* functions, and transparent stencil.]

Vol. xxviii., no. 6. J. R. KANTOR. 'Association as a Fundamental Process of Objective Psychology.' [The Associationists magnified a single kind of associational process, namely, connections between implicit responses. To an objective and naturalistic psychology associational processes, as (1) the processes of organising connections between stimuli and acts and (2) the operation of these reactions when formed, appear as a fundamental basis of all behavior. Such specific reactions to stimuli as learning, thinking, and the manipulations of skill are all operations of connections between stimuli and responses; and psychological capacities of various sorts may be looked upon as the associations of many responses and stimuli of some given type, all of which in their operation give the individual a series of general competencies serviceable in his various adjustments. 'Laws' thus make way for innumerable contributing conditions. We may, however, distinguish modes (*e.g.*, primary and secondary, temporary and inseparable, original and formed, logical and conventional, direct and indirect) and levels (reflex, unintentional, intentional, critical) of associative formation. Finally, since association consists of the organisations of stimuli and responses, it cannot have a neural basis.] J. E. BOODIN. 'Sensation, Imagination and Consciousness.' [The centres of the central nervous system form a hierarchy of energy-patterns, not storehouses or factories of sensory or imaginal content; the content is due to lines of motion connecting these energy-patterns with the sense-organs. The processes of suppression, selection and integration at the various neural levels are due, not to consciousness, but to the characteristic energy-patterns of the levels. Consciousness need not be limited to a particular level, though we have evidence of it only at the level of meaning-patterns; it contributes, in connection with energy-patterns and their excitement, the bare fact of awareness; it is not an explanatory principle. Individual behavior may be explained in terms of neural patterns and their stimulation; the concept of mind, as a system of intersubjective meanings or valuations and of controls as resulting therefrom, becomes necessary only when we take account of social relations.] H. C. WARREN. 'Some Unusual Visual After-Effects.' [Reports delayed after-sensations; a prolonged after-sensation of glare; and observations of voluntary and involuntary visualisation. The experiences are brought into relation with those of Urbantschitsch and Jaensch.]

Vol. xxix., no. 1. W. MCDOUGALL. 'Prolegomena to Psychology.' [The aim of psychology is to render our knowledge of human nature more exact and systematic, that we may control ourselves more wisely and influence our fellows more effectively. Psychology has three direct sources of information: introspection, the noticing of one's own experiences; observation of the conditions or occasions of experiences; observation of the expressions of experiences. These three means of approach have been used with varying emphasis, with varying motivation, with various supporting hypotheses; thus the historical school-systems have arisen. Today the student is confronted by a number of mechanistic readings of psychology (behaviorism, sensationism, associationism, presentationism). But the fundamental principles of these psychologies are, at the best, matters of overhasty dogmatism. We cannot avoid the concept of 'mind,' as that which expresses itself in the experience and behavior of the individual organism; and we must begin our psychology by gathering all possible facts of human experience and behavior and by inferring from them the nature and structure of the mind. Psychology thus becomes the science, empirical and positive, of the human mind. At the outset we note (1) that experience is always of

something and of some one, so that whenever we refer to a fact of experience we imply some one thinking of something, and (2) that it is necessary to distinguish clearly facts of mental activity and facts of mental structure.] E. C. TOLMAN. 'A New Formula for Behaviorism.' [Advocates a non-physiological behaviorism, to be worked out in terms of stimulating agency, behavior-cue, behavior-object and behavior-act. The theoretical advantage is that all the various methods of actual psychology are thus brought under a single rubric; the practical advantage, that the treatment of motive, determining tendency, purpose, etc. becomes easier and more adequate.] A. A. ROBACK. 'Intelligence and Behavior.' [A survey of recent definitions shows that behaviorism has not furthered our knowledge as regards the problem of intelligence. This remains primarily what it always was: the problem of understanding or comprehension. The intelligent man is he whose higher mental operations are as a rule carried out on some more or less consistent plan.] G. S. GATES. 'The Meaning of the Term "Practice",' [Practice as defined may be taken to mean mere repetition, repetition with gain, a certain type of consciousness, phenomena or processes occurring in the individual when an act is repeated. It is best to adopt the widest meaning, of mere repetition of any process or exercise of any function; further enquiry may then proceed from this basis. Customary practice-phrases must necessarily be altered.] H. R. CROSLAND. 'Conscious Analysis in Learning,' [Critique of Perrin. Conscious processes are criteria for the presence of certain cortical integrations and the workings of integrated mechanisms, which frequently can be known in no other fashion.]

Vol. xxix., no. 2. M. F. WASHBURN. 'Introspection as an Objective Method.' [The first doctrine of a radical behaviorism, that consciousness does not exist, is traversed by the existence of sensory qualities. The second doctrine, that psychology should abandon introspection, is less easily met. But introspection may be redefined, from the behavioristic point of view, as symptomatic language behavior. So considered, it (1) yields results as trustworthy as those accepted in other descriptive sciences, which themselves, indeed, often rely upon introspective evidence. It is most vulnerable in cases where the reactions reported by language alone have no immediate external stimulus. Moreover, it (2) has furnished data which the behaviorist cannot afford to disregard; which, in fact, he takes for granted. Witness Sherrington's appeal to observations on flicker; Head's appeal to the Weber compass experiment; and, most striking instance of all, the behaviorists' own theory of learning: it is to introspection that we owe our knowledge of kinaesthesis, or (in behavioristic phrase) it is the evidence of language reactions associated with the contractions of the muscles of various parts of the body which is most important for the theory that habit and association rest on a kinaesthetic or proprioceptive basis.] J. A. MELROSE. 'The Crux of the Psychological Problem.' [The central problem of psychology is the problem of conscious 'thinking' reflection. Over against it, the older psychology has the theoretical, behaviorism the practical advantage. The former, nevertheless, has failed to produce a generally accepted theory, and the latter may be improved out of its doctrinaire intellectualism. Let us admit, as behaviorists, that the sign-system structure of the reaction is more important to the organism's expression and more accessible to science than any muscular reaction; that distinctively human behavior is the adaptive use of a vocal sign-system known as language. Then the problem stands clear before us.] F. L. ALLPORT. 'A Physiological-Genetic Theory of Feeling and Emotion.' [The craniosacral division of the autonomic, supplemented at times by the cerebrospinal system, produces the visceral responses represented in consciousness as pleasantness; the sympathetic division, those represented as unpleasantness. The differentiae of emotions within a single affective class are to be sought in the sensations arising from stimulation of the proprioceptors in the muscles, tendons, joints of the somatic part of the organism.] E. C. TOLMAN. 'Con-

cerning the Sensation Quality: a Behavioristic Account.' [The world of stimuli presents for every organism a particular system of likenesses and differences. These may be registered, behavioristically, in term-characters. Never, not even in the case of our fellow-man, can the immediately experienced qualia be known.] J. L. MURSELL. 'The Stimulus-Response Relation.' [Both behavior psychology and self psychology are working hypotheses, whose representatives have no necessary quarrel. In the stimulus-response relation, taken broadly, we have an adequate basis for an objective psychology: against this position current criticism (Warren, Calkins) is ineffective.]

Vol. xxix., no. 3. J. L. MURSELL. 'The Ontogenetic Significance of Instinct, Habit, and Intelligence.' [Instincts constitute the basic tendencies or impulses of the individual, and can be modified but not eliminated by habit. Habit is essentially a matter of function. Its value is that it adapts original tendencies to a complex environment, and makes action more 'intelligent,' *i.e.*, more efficient and successful. This is the normal ontogenetic sequence.] C. LADD-FRANKLIN. 'Practical Logic and Color Theories.' [Certain recent theories of color present instructive instances of sins against fundamental methodological principles. Troland's theories are taken in evidence, and the author's theory is defended against Troland's criticisms.] L. T. TROLAND. 'The Significance of Psychical Monism for Psychological Theory.' [Exposition, with bibliography. The world of external reality is a complex system of psychical entities generally similar in character to the internal system which comprises any thinking individual. The system of physics is an unfinished product of the efforts of such an individual to formulate the abstract relations existing between the components of this external system. The physical scheme is thus formally identical with the external psychical scheme, but its terms or qualities are different.] R. H. WHEELER and T. D. CUTSFORTH. 'Synaesthesia, a Form of Perception.' [In the case examined, that of a blind subject with stereotyped synaesthetic imagery, the secondary process is essential to perception; the primary process by itself is only an indescribable something. Sensation develops to perception by way of an attention-shift from either process to the other; the simplest describable mental process is thus a dual phenomenon.] T. R. GARTH. 'A Comparison of Mental Abilities of Mixed and Full-Blood Indians on a Basis of Education.' [Groups of mixed (165) and full-blood (215) Indians, of the same school-training as indicated by grade attainment, were given the same tests (of higher mental processes) under controlled conditions. The mixed excelled the full-bloods by about 11%; the best scores were about evenly divided. The difference may, after all, be nurtural and not racial, since pure-blood thinking must meet interferences from which mixed-blood thinking is relatively free.] Discussion. V. S. YARROS. 'Induction and Radical "Psychology".' [Not all radicals are made such by thwarted ambition: the idealism of youth and the clear-sightedness of adult thought play their part, as well as bias and personal grievance. An inductive study is needed.]

Vol. xxix., no. 4. S. I. FRANZ. 'Psychology and Psychiatry.' [Theoretically, the gap between psychology and psychiatry is bridged by abnormal psychology and psychopathology; but as intermediate technologies with special groups of workers these disciplines do not yet exist. Since in both psychology and psychiatry a knowledge of abnormalities and of pathological conditions is of value, investigation in the middle field should be urged; the oftener the extremes are brought into contact, the better also will be the mutual understanding of their representatives.] B. H. BODE. 'What is Psychology?' [The psychology of mental states is moribund; mechanistic behaviorism lets the distinctively psychological evaporate. The only hope lies in a psychology of purposive behavior, whose chief concern is

with the unique and irreducible nature of the stimulus.] W. B. PILLSBURY. 'Suggestions for a Compromise of Existing Controversies in Psychology.' [The outstanding differences in the psychology of today are really the age-old differences of animism and materialism. They are not harmonisable, and not amenable to investigation, because they are rooted in something like instinct. At best there may (as in the days of Wundt and James) be a specious truce, which is inevitably to be broken.] J. R. KANTOR. 'An Analysis of Psychological Language Data.' [Objective psychology has to set linguistic reactions in their proper perspective in relation to the other coordinating functioning responses to stimuli. Language responses are inherently indirect or referential adaptations to stimuli, involving an adjustment stimulus (acted on or reacted to) and an auxiliary stimulus (person using language). Language and thought are not identical; they constitute different types of adaptive response and they operate differently; thus, language is always indirect, thought always direct adaptation. Language reactions may be divided modally into morphological and functional; the latter are either expressive or communicative. As communicative, language reaction systems may be primarily transmissive (*e.g.*, vocal speech) or primarily receptive (*e.g.*, reading and understanding). Words, to be psychological data, must be either stimuli or responses.] L. M. TERMAN. 'A New Approach to the Study of Genius.' [Impressionistic and anecdotal methods (Lombroso) gave way to inductive and statistical study of biographical material (Galton), replaced in its turn by investigation of the living (Cattell). The next step is the study of genius in the making, of gifted children. The writer reviews the work done and doing at Stanford University, and gives a few tentative conclusions: in the schools of larger cities, 1 pupil in 250 has an IQ above 140; Jewish cases are in excess; some cases are of very inferior social status, etc.] R. O. FILTER. 'A Practical Definition of Character.' [Intellectual and aesthetic values have been ruled out of the concept of character, and moral values are now following them. The emphasis is on force rather than on direction of activity, on quality of behavior as strong, persistent, ready, rapid, etc., rather than on its value as good, right, wise, etc.] Discussion. F. A. PERRIN. 'Conscious Analysis, Introspection, and Behaviorism.' [Reply to Crosland.] E. B. TITCHENER. 'The Term "Practice."' [Reply to Gates.]

Vol. xxix., no. 5. A. P. WEISS. 'Behavior and the Central Nervous System.' [Behaviorism studies the origin and development of those bodily movements (reactions) of the organism which establish its social status. Warren's limitation of the behaviorist to a study of the responsive phase of the sensory-cerebro-motor system is inacceptable, since behaviorism takes account of the stimulating conditions from the standpoint of the social categories into which they may be classified, and of responses from the standpoint of their significance as stimuli (signals) in establishing the social status of the organism. In general, there is no bridge between the two doctrines; the gap will probably grow wider.] Z. Y. KUO. 'How are our Instincts Acquired?' [Bodily movements (units of reaction), chaotic at birth, are presently integrated, simultaneously and temporally. Behavior-sets, developed through the establishment of conditioned responses, serve to explain purposive behavior in purely objective and mechanistic terms. They help us to classify temporal integrations according to the end-results accomplished; and in classifying these integrations we classify the instincts.] J. PETERSON. 'Intelligence and Learning.' [The trend of psychology toward intelligence-tests has brought theoretical problems along with practical results. We assume, *e.g.*, but we do not know, that there is practically perfect correlation between degrees of ability to learn the habits and ideas on which the tests are based and degrees of intelligence. Another problem is the general nature of mental growth. It is, again, a grave error to identify the IQ with intelligence; it is gravely confusing to identify outright learn-

ing ability with intelligence. No doubt, the two are related; but we need a more functional analysis of both. The older passive associationism, against which the pseudo-psychology of the subconscious marks an unreasoned revolt, must be set in its proper place by emphasis on the inner urge of the metabolic changes as important factors in instincts. Only a dynamic psychology can tell us how errors become eliminated in trial-and-error learning, while certain acts finally survive in habits.] J. A. MELROSE. 'The Organismal Point of View in the Study of Motor and Mental Learning.' [The approach must be made by way of the total organism and the organism's most characteristic behavior. Thus, if a definite constant pattern is found in the behavior of language-use (characteristic of man), which is unaccounted for whether by lower types of learning or by the constants of the environmental situation in which language-habits are formed, then this pattern must be due to man's inherited structure. As definitely correlated with that structure, the constant pattern would represent the structural difference between thinking and animal (motor) learning.] Discussion. C. SPEARMAN. 'A Friendly Challenge to Professor Thorndike.'

Vol. xxix., no. 6. S. W. FERNBERGER. 'Behavior *versus* Introspective Psychology.' [The sciences of consciousness and of behavior are separate and independent. The older psychology then becomes a speculative procedure attempting to correlate the data of these two sciences with each other and with physiology.] H. N. WIEMAN. 'The Unique in Human Behavior.' [Minor units of behavior are integrated into (animal and human) instincts; instincts are integrated into (specifically human) interests. Instinct adapts the organism to a specific situation; interest adapts it to an ever more inclusive situation, making simultaneously every possible contact with environment. Hence arise the unique behavior-traits of philosophy, religion, science, art, love.] R. H. WHEELER. 'Analyzed *versus* Unanalyzed Experience.' [Reply to Calkins. The data of Ach's and Michotte's investigation were unanalyzed meanings; those of the author's, analyzed meanings. Illustrations are given.] C. M. CAMPBELL. 'What can Psychology Contribute to our Knowledge of the Mechanism of Mental Disorder? With some Comments on the Term "Depression." ' [Confronted with a case of morbid depression, the psychiatrist finds no useful formula already prepared by biochemist, physiologist or psychologist. Analysing the patient's own account, he comes upon a very complicated biological situation, in which were present unsatisfied cravings, vague yearnings, organic desires, repressed fantasies, stifled irritiations, strivings toward religious solutions. Psychology should evidently train its students in the analysis of complex emotional reactions.] C. E. FERREE and G. RAND. 'The Effect of Variations of the Intensity of the Illumination of the Perimeter Arm on the Determination of the Color Fields.' [With very high intensities of stimulus the limits of R, Y, B are coincident with the limits of white-light vision; those of G are less wide. With medium intensities, and the relative energies of the prismatic spectrum of a Nernst filament, the limits (concentric) are, from widest to narrowest, R, Y, B, G. With medium intensities of equal energy the limits of R, Y, B interlace and crisscross; those of G are narrower. For pigment stimuli we obtain, according to special conditions, zones either interlacing or concentric in the order, widest to narrowest, R, B, G or B, R, G.] J. M. RIFE. 'Types of Dextrality.' [There are three types of right-handedness with three corresponding types of left-handedness. The first type comprises those whose bimanual preference is divided between R and L. The second includes those whose bimanual is the opposite of their unimanual preference. The third embraces those whose unimanual and bimanual preference is the same. Unless these types are distinguished, misstatements of fact are likely.] H. C. WARREN. 'The Significance of Neural Adjustment.' [The attempt to describe organic reactions solely in terms of the initial and terminal data (stimulus and re-

sponse) ignores the central factor of neural adjustment, which the writer regards as the key to the entire process.] Discussion. M. W. CALKINS. 'The Affiliations of Behaviorism.' [Comments on Washburn and Mursell.]

Arch. f. d. ges. Psychologie. Bd. xliii., Heft 1. F. KIESOW. 'Ueber Metallglanz im stereoskopischen Sehen.' [Under the conditions laid down by Rood in 1861 it is possible to produce a true metallic lustre in stereoscopic vision. This fact does not invalidate Kirschmann's theory of lustre (vol. xli.), since the effect is due to assimilative processes.] F. KIESOW. 'Ueber die taktile Unterschiedsempfindlichkeit bei sukzessiver Reizung einzelner Empfindungsorgane.' [Experiments with successive stimulation of the Meissner corpuscles of the hairless wrist give a constancy of the *DL* between the limits 3 and 6 gr/mm at one-seventh; above and below (1 to 8 gr/mm) there are slight deviations. The writer describes a new hair-aesthesiometer.] G. STOERRING. 'Zur Psychologie der Erinnerungsgewissheit.' [Experiments on the reproduction by drawing, immediately and at weekly intervals for five weeks, of meaningless forms exposed for 1, 3, 6 or 10 min. The principal factors in the assurance of recollection are (1) verificatory reproductive processes and (2) the consciousness of insistence (*Sichaufdrängen*) whether of the idea of object or of the observer's condition during impression. This insistence is very like the insistence which, in processes of judgment, is directly connected with the consciousness of validity. Hence there is a strong tendency, when it appears, toward a reproduction of the consciousness of assurance; there is no actual assurance of recollection. Strongly visual observers report an insistence of the idea of object itself; but probably there is here an unremarked consciousness of insistence.] S. FISCHER. 'Ueber das Entstehen und Verstehen von Namen, mit einem Beitrage zur Lehre von den transkortikalen Aphasien,' ii. [Our experience in understanding a word is made up of (*a*) the auditory perception of the sound-complex with passive direction upon the object of perception; (*b*) a quality of familiarity; (*c*) either a consciousness of direction or an intentional experience that points either to a sphere of meaning or to a knowing; (*d*) thereupon (or concomitantly) either an experience of knowing or a consciousness of sphere. Understanding is now complete, though (*e*) illustrative palpable ideas may appear. Our experience in naming a familiar object is made up of (*a*) the experience of the perceptive content with passive direction upon the object of perception; the relations whereby the object was first apprehended may be dispositionally present; (*b*) a quality of familiarity; (*c*) the actualisation of the sound-complex by associative reproductive-tendency; (*d*) earlier (or concomitantly) an intention toward the object of perception, whereby the sound becomes denomination. The two processes are radically different. So far, then, as the 'concept' (*Begriff, Begriffsfeld*) of the aphasic schemata of Wernicke-Lichtheim and Goldstein confuses them, so far are these schemata themselves psychologically unacceptable.] E. MALLY. 'Lineare Regressionen und mittleres Verhältnis.' [Wirth's mean ratio, in the sense of the mean line, gives a fixed relation between the observed magnitudes that has been, so far as possible, freed of their accidental variations and is reduced to the 'normal conditions' of the uncontrolled influences. Another mean ratio, in the sense of the mean regression,—a value demanded by the calculus of correlation,—gives a more direct description of the factual status, since it shows how, with the given 'accidental' distribution of the values, the deviations are on the average interrelated. The two values themselves are definitely related.] Literaturbericht: Referate. W. WIRTH. 'Zur Kritik einer verstehenden Psychologie der Weltanschauungen.' [Criticism, general and special, of K. Jaspers' "Psychologie der Weltanschauungen."] S. FISCHER. 'O. Bumke, Zur Diagnose der Geisteskrankheiten; S. Freud, Zur Psychopathologie des Alltagslebens.'

NOTES

The Expression of Simple Feeling

The occasion of this Note is a quotation recently made from one of our Minor Studies: a quotation which, though literally accurate and made, of course, in all good faith, must nevertheless be misleading to a reader who has not the original paper very clearly in mind. The circumstances are as follows.

In 1921 Young drew from experimental results the conclusion that P, as contrasted with U, is "organically-kinaesthetically negative." He qualified this conclusion by the phrase "so far as our data go," and by the remark that "any result is a function of conditions."[1] It seemed to us that it would be fairly easy to set up conditions that should show P as "organically-kinaesthetically" positive: and Corwin published our results later in the same year.[2]

It is from Corwin's paper that the quotation is taken to which I just now referred. "It is impossible, therefore, to accept Corwin's interpretation," so Young now writes, "that 'the direct response of the organism to P is .... a definite activity of pursuit or of tendencies to pursuit.'"[3] When I read this quotation, I wondered that any student of mine should have made the statement, and not less that I should have allowed it to appear in print. In point of fact—though the quotation is literally correct—Corwin said something very different: so different that Young himself, had he not been prepossessed by his own argument, would have seen that the abbreviated sentence is misleading. Corwin wrote: "The direct response of the organism to P is, as stated above, either relaxation *with a certain degree of expansion*, if the stimulus is weak or stationary; or, if the stimulus is intense, and if the source of the P is withdrawn, a definite activity of pursuit or of tendencies to pursuit." The occurrence of the 'activity' is thus doubly qualified: the stimulus must be strong, and must also be in course of movement away from *O*. It was precisely our point that the result was a function of its conditions; and we were not 'interpreting' but recording.

The words in italics were so printed in the original paper. Every set of psychological experiments is likely to raise issues of systematic interest; and such an issue is here in question. A Minor Study is not, however, the place for systematic discussion: we were content with the bare suggestion that 'expansion' and 'contraction' might be better terms, in this particular context, than 'pursuit' and 'avoidance,'[4] and with the indications afforded by the reports of our *O*s. If we had been led into further discussion we should hardly, I think, have mentioned the teleological theories of feeling without reference at any rate to Külpe,[5] or the movements of sensory attention without reference at any rate to Jastrow.[6]

E. B. T.

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[1] P. T. Young, Pleasantness and Unpleasantness in Relation to Organic Response, this Journal, xxxii., 1921, 53.

[2] G. H. Corwin, The Involuntary Response to Pleasantness, *ibid.*, 563 ff.

[3] P. T. Young, Movements of Pursuit and Avoidance as Expressions of Simple Feeling, *ibid.*, xxxiii., 1922, 522.

[4] On the general question see also E. B. Titchener, Text-book of Psychology, 1910, 487.

[5] O. Külpe, *Grundriss der Psychologie*, 1893, 276 ff.

[6] J. Jastrow (with the assistance of Helen West), A Study of Involuntary Movements, this Journal, iv., 1892, 398 ff.; Fact and Fable in Psychology, 1900, 312 ff.

Professor Moore's "Note on 'Affirmation and Negation'"

Professor Moore's Note in the July, 1922 (33, 449 f.) number of this Journal, in which he criticizes my report of experiments on "Affirmation and Negation" in the January, 1922 (33, 84-96) number, seems to call for a reply. Professor Moore agrees that my data show that a positive judgment (or affirmation) requires less time than a negative judgment (or negation). Then he says: "To infer further from this, however, that consequently there is a psychological *opposition* between a logical affirmation and a logical negation is hardly warranted." But nowhere, in the article referred to, was any such inference made.

His closing sentences also deserve consideration. "In other words, the only *opposition* between affirmation and negation seems to be a *logical* opposition: the only *psychological* difference is a difference in *duration* of process, not an opposition at all. I do not see, therefore, that Dr. Griffitts has proved his point as a result of his experiments." As a matter of fact, an attempt to prove any such point was so foreign to what I actually tried to do that in my article nothing at all was said regarding the possibility, even, of there being any such opposition. My aim was merely to determine whether or not there are any differences—conscious or not—in any phase or aspect of affirmation and negation. I explicitly stated that I was interested primarily in the objective results, and that I therefore took pains to prevent any introspective tasks from interfering with the securing of valid objective data.

Nevertheless, I did secure some introspective data, which I am ready to admit are in themselves inadequate; Professor Moore may therefore be justified in overlooking or ignoring them. If, however, these introspective reports have any value, they indicate differences in the conscious aspects of the processes of affirmation and negation. He admits only the differences in duration, while I believe there are other differences; and the careful reader will find that a good deal of direct and indirect evidence of such differences was reported. But these neurological and psychological differences do not imply neurological or psychological *opposition*.

On p. 86 l. 4 of my article, the expression "made two reactions" should read "made two wrong reactions."

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The Stern Variator

In 1906 Stern[1] published a note saying that the manufacture of his "Tonvariator" had been transferred from F. Tiessen at Breslau to Max Kohl at Chemnitz. Stern does not say that any change had been made in its structure. The Kohl instrument, however, does not accord with the variator described by Stern[2] in 1902. As made by Tiessen, every degree of arc drawn on the dial of the spiral which lifts the piston represented a change of pitch, say, of 2 vib. The spaces on the dial were all equal, because the piston was set at positions giving equal increments of change in rate of vibration. The equality of spaces made it possible to gear a micrometer to the spiral, by means of which the dial could be set to a fraction of a space and the piston, consequently, could be set to a fraction of a vibration. These adjustments are all specified in Stern's description of 1902. The variators in the Cornell laboratory, however, made by Kohl, do not fulfill the requirements. The spaces on the dial which represent unit-

[1] L. W. Stern, Notiz über den Tonvariator, *Ztsch. f. Psychol. u. Physiol.*, 40, 1905-06, 304.

[2] *Id.*, Der Tonvariator, *Ztsch. f. Psychol. u. Physiol.*, 30, 1902, 422-432.

changes in vibration frequency are not equal. This departure from the specified form renders the micrometer useless and interpolation uncertain. If the variators owned by the Cornell laboratory are like others made by Kohl, they are definitely inferior to a variator made according to Stern's specifications. Apparently, Kohl cut a spiral which had approximately the proper slope, set the piston by tuning, turned the spiral until it supported the piston, and then marked the position on the dial attached to the spiral. Any radius which was too short would widen the space between it and the next lower setting, and would narrow the space between it and the next higher setting. A long radius would have the opposite effect. With all radii slightly inaccurate, a haphazard set of unequal spaces would result; and these are what the Kohl dial has. Nevertheless, the instrument is furnished with a micrometer, just as if the spaces were all equal. If made according to Stern's description, it would be necessary to set the piston, to turn the spiral forward the proper number of degrees, and then to cut the radius at that point in the spiral to the length which would just support the piston. It is in this more elaborate and more accurate calibration that the Kohl instruments fail.

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On The Anthropological Find in Rhodesia

I. *The Finding of the Broken Hill Skull: The Mystery of the Great Bone Cave.* Three reports by: (*a*) William A. Harris, (*b*) A. Smith Woodward, (*c*) Arthur Keith. *Illustrated London News*, 159, 1921, 680-687.

II. *Le crâne néanderthalien de Broken-Hill.* By L. Capitan. *Revue anthropologique*, 32, 1922, 42-47.

III. *The Skull from Broken Hill in Rhodesia.* By G. G. McCurdy. *Am. Anthropologist*, 24, 1922, 97-98.

The "Broken Hill Skull" was found ninety feet below the ground surface in Bone Cave, which has been opened up by the operation of the Rhodesian Broken Hill Development Co., Broken Hill, Northern Rhodesia (near Victoria Falls), So. Africa. Hundreds of tons of bones of small animals now existing in Africa had been removed from the cave. The skeleton, the only human remains found, was unearthed by mine laborers at the extreme interior part of the cave. There was created sufficient interest among the laborers by this for them to send the skull to the "white boss," after which they proceeded to break up the skeleton for mining purposes. There were recovered: a leg bone, collar bone, portion of shoulder blade, portion of pelvis with coccyx attached, a part of the lower jaw, various parts of other bones not identified, and most of the mineralized cast of the body (I, *a*, 680).

The find is undoubtedly human. The measurements by Keith place its brain capacity between that of the average Englishman (about 1480 c.c.) and that of the skull of *La Chapelle-aux-Saints* (Neanderthal race) with a capacity of 1600 c.c. Keith is of the opinion that, even allowing for its great projections of brow-ridges and for possible extra thickness of the bone, it could not fall far short, in point of size of brain, of that of average modern man. The *Pithecanthropus* represents a much smaller skull than this fossil. Woodward attempted to fit the Heidelberg jaw, the largest known human fossil jaw, to this skull, finding it both too short and too narrow. Piltdown has an apelike palate, while the palate of the Broken Hill skull is typically human, "adapted to perfect speech, and bounded by

the horse-shoe shape row of large, though absolutely human, teeth" (I, *b*, 682). Keith considers the palate to be less ape-like than even that of the modern negro.

This Rhodesian man had a prominent heavy face surmounted by large brow-ridges, brow-ridges as heavy as, if not heavier than, Neanderthal. The bones of the face are considered by Woodward to be typically human; but their arrangement indicates that the face would be broad and flattened. It is possible that the lower jaw was more prominent than that of Neanderthal man. Woodward states that there are "reminiscences of an apelike ancestor in its face" (I, *b*, 682).

The *foramen magnum* is as far forward as in modern man," and it shows that the skull would be perfectly poised on an upright trunk" (I, *b*, 682), says Woodward, while Keith considers that there would be a hafting of the skull as in Neanderthal man.

The teeth are worn in such a manner as to prove that the lower teeth met the upper edge to edge, a feature common in the dentition of early races. The wisdom teeth are much smaller than the other grinders. Caries had affected the teeth and the bones surrounding the teeth were diseased. It is the opinion of both Woodward and Keith that disease of the teeth is a modern affliction. The teeth diseased were those most likely to be attacked by disease in modern times. Woodward states: "So far as I am aware, this is the only example of caries in teeth of a prehistoric skull hitherto discovered" (I, *b*, 682), while McCurdy makes the contradictory statement: "the teeth had suffered considerably from caries, but perhaps no more so than was the case with the skull from *La Chapelle-aux-Saints*" (III, 98).

This fossil find has not been studied sufficiently as yet to determine absolutely its geological age and race. McCurdy's conclusion is that "with due allowance for variation it would seem reasonable to assume that the man from Broken Hill is a variant of the Neanderthal type" (III, 98). There are paradoxical characters in the skull. The cranial vault is raised, more like man than the Neanderthal. However, this same character appears in *Pithecanthropus*. The profile of the face has very much the aspect of Neanderthal. Keith concludes that this fossil found in Rhodesia "agrees in most features with . . . . Neanderthal man" (I, *c*, 686), and Capitan summarizes the consensus of opinion to the effect that this skull in all probability is a Neanderthal, but that it is impossible to state exactly.

Woodward does not go so far as to place the Rhodesian man in the Neanderthal group, even considering it of extremely wide variation. His "impression is that the Rhodesian man is of later development than Neanderthal man, of more recent geological date. He is one of the multitude of extinct races yet to be unearthed, who completed their mental equipment before undergoing the change which we regard as refinement of the face" (I, *b*, 682). Woodward bases his contention mainly on the fact that, while Neanderthal's limb bones allowed only for inelegant shoulders and a shuffling gait, the Rhodesian man was found with bones which are in all respects those of ordinary modern man and of a size which would indicate that he stood upright at a height of about six feet. Woodward is the only anthropologist among these writers who appears to take into consideration bones other than the skull.

It is evident that the Broken Hill fossil did not live at so remote a period as known fossil Neanderthals. Capitan raises the question whether he may be a late descendant of the Neanderthal race; holding, furthermore, that it is possible that some Neanderthal descendants may be living in remote corners of Africa to-day. Ten years ago there were found implements of Mousterian culture near Victoria Falls. There have been made numerous such finds in Africa. Capitan considers that we have made our first find of the man who is responsible for these implements.

Because there have been no adequate studies of geological strata in Africa, there is nothing to assist in the placing of Rhodesian man, except European fossils. An ethnological study of Africa might throw some light upon these conjectures. Africa appears to be a valuable source for anthropological exploration in the future.

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MIMICRY IN THE EGGS OF *Cuculus canorus*

(1) Friedrich von Lucanus: Zur Frage der Mimikry der Kuckuckseier, *Journal für Ornithologie*, 69 Jahrgang, Heft 2, April, 1921, 239-258.

(2) Horst Wachs: Zur Aehnlichkeit der Kuckuckseier, *Biologisches Zentralblatt*, 42 Band, Nr. 6, Juni, 1922, 270-278.

That the European cuckoo (*Cuculus canorus*) lays its eggs in other birds' nests is an old and striking observation of popular ornithology, but in the matter of detail even professional ornithologists have long remained in the dark. It has been known, for example, that cuckoo eggs are smaller than might be expected from the size of the bird, and vary considerably in ground-color and markings; but opinion has been divided on the question whether the eggs of the cuckoo resemble the eggs of the birds in whose nests they are laid, some observers asserting that resemblance, if it occurs at all, is rare and accidental, and others that resemblance is the rule. A good deal yet remains to be done before a full explanation of the matter can be offered; but the paper of von Lucanus, noted above, seems to have settled at least the question of resemblance once for all by showing that both resemblance and non-resemblance occur, and approximately in what proportions and in what sorts of nests.

Making use of the great collection of birds' eggs in the *Museum für Naturkunde* in Berlin von Lucanus compared carefully the cuckoo eggs with those among which they had been laid, distinguishing three conditions (close resemblance, tolerable resemblance and no resemblance), and has stated his findings both in concise descriptive paragraphs and in a statistical table. The collection consisted of 728 clutches of eggs from 30 different species with which 765 cuckoo eggs had been laid. The most striking items in the table are the entries for the *Gartengrasmücke* (the Old World warbler *Sylvia borin*) and for the wren, *Troglodytes troglodytes*. The warbler is represented by 481 clutches of eggs with 502 closely similar cuckoo eggs[1]; and the wren by 109 clutches with 120 cuckoo eggs none of which were like wren's eggs. Of the 28 remaining species the number of clutches was much smaller, from one to a dozen or fifteen, and the degree of resemblance varied. With other warblers of the Sylvian genus the resemblance was usually close, while with those of the genus *Phylloscopus* (9 clutches with 10 cuckoo eggs) there were no cases of resemblance. For the rest the figures were mostly scattering, instances of resemblance and its absence often appearing in nests of the same species. If we leave out of account, on the one

[1]Of the 502 cuckoo eggs 453 were extremely close matches with the eggs with which they had been laid. In 49 instances the differences were greater, though still not exceeding the normal range of the *Gartengrasmücke* eggs among themselves, and for this reason were counted by von Lucanus as closely similar. Of the eggs of this species the ground color may be white, dirty white or yellowish, the markings drab, dark brown, reddish, or blue gray and in the form of spots, specks or lines—all of which variety would favor the parasitic tendencies of the cuckoo.

hand, the cuckoo eggs laid in the nests of the *Gartengrasmücke* and, on the other, those laid in the nests of the wrens and the *Phylloscopus* warblers—as tending unduly to weight the results—there remain 133 cuckoo eggs laid in nests of 27 other sorts of birds. Of these 95 or 72% show resemblance, while 38 or 28% do not. Resemblance is present therefore in general in about 7 cases in 10; and it extends in some instances to practical identity in both ground color and the finest details of the markings.

If von Lucanus has settled one question, he has opened up a number of others. How comes it that there should be such consistent resemblance in the case of the Sylvian warblers, and such equally consistent lack of it in the case of the wrens and the *Phylloscopus* warblers? And when there is resemblance, how is the female cuckoo brought to lay in nests containing eggs that hers will match?

The earlier fantastic suggestions that the female cuckoo has the power of determining in advance the color and markings of her unlaid egg in such a way as to make it match those with which it will be deposited, or that the resemblance is due to the food administered by the foster parents to the female cuckoo while still a nestling, are very naturally rejected by von Lucanus in favor of an explanation based on the probable genesis of the habit of parasitic nesting. It seems likely that the ancestors of the European cuckoo began by using a joint nest and brooding in common, as some cuckoos in other parts of the world still do. As time passed the brooding and nest-building instincts weakened in some of the partners; they laid in the joint nest, but omitted to do their share of the rest of the work; *i.e.*, they began to be parasitic on their own species. In their descendants in course of time these instincts wholly disappeared and the habitual use of other birds as foster parents became the price of survival. In selecting nests in which to lay, those nests would be most naturally chosen whose eggs resembled the eggs in the original joint cuckoo nests; for which they might in a manner be taken. A condition similar to this still exists in the case of certain congeners of the cuckoo, which lay eggs of a fixed type in nests of particular species whose eggs theirs closely resemble. The original cuckoo eggs were in all probability very much like those of the *Gartengrasmücke* (*Sylvia borin*), with which they most frequently appear in the Berlin collection (481 clutches with 502 cuckoo eggs). In the earlier stages of its parasitism the cuckoo probably laid in the nests of this species exclusively, a wider variability of its own eggs and a wider range in its selection of nests both arising subsequently. If such were the case, the perfecting of the resemblance could easily be taken care of by natural selection, and distinct varieties of cuckoos would be found imposing their eggs chiefly upon single species, as actually happens in northern Europe where one of the finches suffers and in southern Europe where another warbler (*Sylvia orphea*) is the victim.[2] If at the same time the wrens and the *Phylloscopus* warblers accepted foreign eggs without protest, the lack of resemblance in their cases would also be accounted for.

The data on the tolerance shown by birds of different species toward foreign eggs appear not to be very extensive. Such studies as have been made show great differences among birds of different species and even among different individuals of the same species in this particular, some accepting eggs that differ markedly in size and color from their own and others rejecting those showing but small differences. In view of such variability in the selective factor, von Lucanus is inclined to attach small

[2]For the operation of natural selection it would be necessary, on the one hand, that the female cuckoos should tend to lay their eggs in the nests of one particular species (the species in the nests of which they had themselves been reared or some other) and, on the other hand, that dissimilar eggs should be thrown out by the foster parents or the nest deserted.

importance to natural selection as a cause of the resemblances found, holding that natural selection could produce such close resemblances as exist only if *all* eggs that do not match are destroyed, and leaving the possibility open for the operation of other causes.

In the second of the above papers Dr. Horst Wachs summarizes the foregoing study of von Lucanus' and discusses the problem from his own standpoint. The assumption of von Lucanus, that natural selection can bring about close resemblance only in case *all* cuckoo eggs unlike those in the foster parents' nest are rejected, seems to Wachs unnecessary. Rejection of a reasonable portion of the dissimilar eggs would bring about the same result though less speedily. As a matter of fact we probably have all degrees of intolerance of variant eggs and corresponding thereto all degrees of selective action, and consequently all grades of resemblance. The intolerance of the Sylvian warblers has been high and has held the cuckoo eggs close to their original type. Other cuckoos whose eggs varied from this original type have had to look about for nests having eggs like their own; for Wachs holds that the female cuckoo does not lay an egg to match those in the nest but, laying eggs of a certain sort, she hunts for a nest with eggs like her own. In his opinion "every individual cuckoo, as each individual bird, knows its own egg." ("Denn ich halte mit Bestimmtheit dafür, dass auch das einzelne Kuckucksindividuum wie jedes Vogelindividuum sein Ei *kennt*.") So far as she is successful, there might easily arise strains of cuckoos externally similar but differing in egg-type and in the nests in which they lay—if the female cuckoos were parthenogenetic! The perpetual crossing with males of other strains (since there is no reason to suppose that the mating would be selective in the same way) would, however, produce a continual supply of widely varying egg-types. Females laying eggs of these unusual types would have to hunt for appropriate nests and eggs, would often fail to find them, and would be obliged to lay in nests where the eggs were different, with corresponding risk of their destruction. We should therefore expect to find in any regional collection many instances of nests containing unlike eggs, their frequency depending on (1) the number of cuckoos of variant egg-type; (2) the tolerance of the foster parents; and (3) the thoroughness with which the region has been studied with this point in view.

These considerations cover pretty well the cases of resemblance and scattering non-resemblance. What of those of uniform non-resemblance—the cuckoo eggs in the nests of the wrens and *Phylloscopus* warblers? For the former Wachs offers a solution; for the latter, in which the number of instances is small, he thinks that data for an explanation are not yet at hand.

The critical point in case of the wrens is the character of their nests. Cuckoo eggs can not be laid in wrens' nests in the ordinary way, but must first be laid elsewhere and then be carried to the nest and be placed in it by the female. Since the body of the female cuts off the light when she puts her head into the opening of the nest, she has no chance to see the wren's eggs and to be influenced by their likeness or unlikeness—in a word she has to 'go it blind;' and the wrens for their part either do not notice the strangeness of the egg, or have difficulty in removing it, or are highly tolerant of it. It is also Wachs' belief that cuckoos tend to lay in the nests of the same species as that in which they have themselves been reared. In case of open nests this tendency must operate jointly with that to matching of the eggs. In the case of the cuckoos hatched in wrens' nests this latter factor does not come into play. They choose by nest-form only and thus more simply and easily. This would account for the relative frequency of cuckoo eggs in wrens' nests, as the inability of the female to see the wrens' eggs would account for the lack of resemblance.

Taking all the factors into account we seem to have in this matter of the resemblance or non-resemblance of cuckoo eggs, as Wachs well says, a most interesting biological experiment in process, and perhaps a case of the genesis of varieties taking place so rapidly as to bring the beginning and end within the observational lifetime of a single naturalist.

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Wilhelm Wundt Gedaechtnisheft

The *Deutsche Philosophische Gesellschaft* publishes, as Heft 3-4 of the second volume of *Beiträge zur Philosophie des deutschen Idealismus*, a *Würdigung* of Wilhelm Wundt. The contents of the Heft (80 pp.) are: F. Krueger, Wundt als deutscher Denker; P. Petersen, Die Stellung der Philosophie Wundts im 19. Jahrhundert; F. Sander, Wundts Prinzip der schöpferischen Synthese; A. Kirschmann, Wundt und die Relativität; H. Volkelt, Die Völkerpsychologie in Wundts Entwicklungsgang; O. Klemm, Zur Geschichte des Leipziger Psychologischen Instituts. The Heft may, apparently, be bought separately; no price is given.